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AND

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AMERICAN SOCIETY OF CIVIL ENGINEERS

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PAPERS

EARTHQUAKE RESISTANCE OF ELEVATED WATER-TANKS

By Arthur C. Ruge,[1] Assoc. M. Am. Soc. C. E.

Synopsis

The problem of building elevated tank structures that are truly earthquake-resistant is considered in detail; and a new type of construction, embodying increased flexibility combined with snubbing action, is suggested as a practical solution. Extensive model studies, directed toward obtaining engineering data for design purposes, are described and the results summarized. The writer concludes that the commonly used statical method of design against earthquakes cannot be applied safely to elevated tank structures, and that the suggested new type of construction has a safety factor in stress of at least 2 over the standard type of tower. Experiments in which reproductions of actual earthquake motions were applied to a model of a 60 000-gal, 100-ft structure show that dangerous stresses are not avoided by designing against a statical horizontal force of one-tenth the weight of the tank and contents. Rules are given for designing structures of the new type suggested, and reasonable limits of applicability are set.

Introduction

Purpose of Paper.—Consideration of the large number of failures and near failures of elevated water-tanks during the earthquake of March 10, 1933, at Long Beach, Calif., has led to serious study of the earthquake resistance of these structures by many engineers. The importance of the problem is already so well known as to require but passing comment. The actual money value of the structures is in itself not so very important; it is in connection with the fire, life, and health hazards generally accompanying earthquakes that the effective value of a water supply system increases out of all proportion to its actual cost.

A very important class of elevated tank structures is that comprising the gravity sprinkler-supply systems for industrial plants and for many

Note.—Discussion on this paper will be closed in **September, 1937**, *Proceedings*.

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other buildings of moderate height. Elevated sprinkler tanks generally vary in capacity from 50 000 gal to 150 000 gal and in height from 75 ft to 150 ft. This paper is largely concerned with structures that vary within these ranges of size, but more specifically with independent tank structures of all-steel construction. Some of the resuts, however, obviously apply also to structures of reinforced concrete, wood, or mixed construction.

The purpose of this paper is to present the principal results of an extended series of model studies which were directed toward: (1) The determination of the stresses produced in elevated water-tank structures by earthquake motions; (2) the determination of the effect of strengthening the structures by present methods of statical design; and (3) the investigation of a new type of tower design which was developed in the course of the research. That the experiments described herein were restricted to a small part of a very large and important field will be obvious to the reader. Without a comparatively large fund at hand for the work, practical results could be obtained only by narrowing the range of the research.

Nature of the Problem.—At first glance, the design of an earthquake-resistant elevated water-tank seems to be a simple engineering problem. It is not uncommon to find practical engineers reasoning that, since competent engineers can design buildings of six or eight stories to resist moderate earthquakes with considerable confidence, the design of a water-tank tower should present no difficulty, since it is a much simpler structure and of a mass far inferior to that of a building. This kind of reasoning, unfortunately, is the result of a misconception to the effect that a relatively limber, top-heavy structure such as an elevated tank will behave dynamically like a low, rigid building. Indeed, the practical treatment of earthquake-resistant design from the standpoint of dynamics has been, for the most part, of a very superficial nature in the past. The tendency has been to simplify the design methods on the basis of intuition without first attempting a rigorous analysis.

First, it is important to realize that earthquakes manifest themselves as inexorable motions, rather than as forces. The expression, "earthquake force", has become so widely used that many engineers conceive of the problem of quake-proof design as one of simple stress analysis in which the "forces" are determined definitely by the violence of the earthquake alone. The "forces" produced by the earthquake motion depend primarily upon the characteristics of the structure suffering the motion. Various frequencies, or periods, are associated with the inexorable motions of an earthquake, and, although the actual motions may be highly irregular, certain periods may sometimes appear more prominently than others.

If a structure is acted upon by a ground motion which has a period five or six times that of the structure, then it is possible, with reasonable accuracy, to compute the stresses in the structure on the basis of the accelerations of the ground motion. In this case, the forces acting upon the structure are taken as the product of the various masses and the ground acceleration. This simple rule, however, cannot be extended to structures such as elevated water-tanks, which commonly have natural periods within

the range of the periods associated with major earthquakes. Here, it is necessary to proceed with extreme caution; nor can one rely upon intuitive reasoning unless it is backed by a thorough grasp of the dynamics of vibrating systems.

The type of tower now in common use was not developed for earthquake resistance, but merely to fulfill the purpose of elevating the water supply on a structure which has to resist a certain intensity of wind force in addition to carrying the vertical load. For this use it is practically ideal; but there is no reason to accept it as the ideal structure for withstanding earthquake motions, which actual experience and repeated laboratory tests have proved it certainly is not.

The dynamic behavior of elevated tanks is not amenable to simple mathematical analysis because of the complications introduced by the action of the water in the tank. The system is analogous to one made up of a gravity pendulum suspended at the top of an elastic vertical frame work (see Fig. 1), except that the water action in the tank is more complex than that of the pendulum. It will be seen subsequently from the experimental results that the water motion exerts a cushioning effect upon the deflections in a tank tower of moderate size.

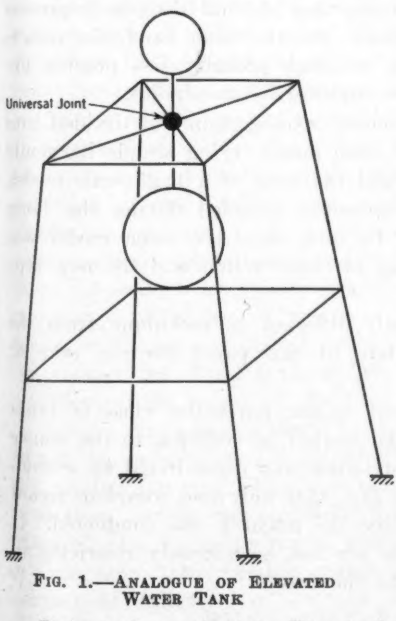

FIG. 1.—ANALOGUE OF ELEVATED WATER TANK

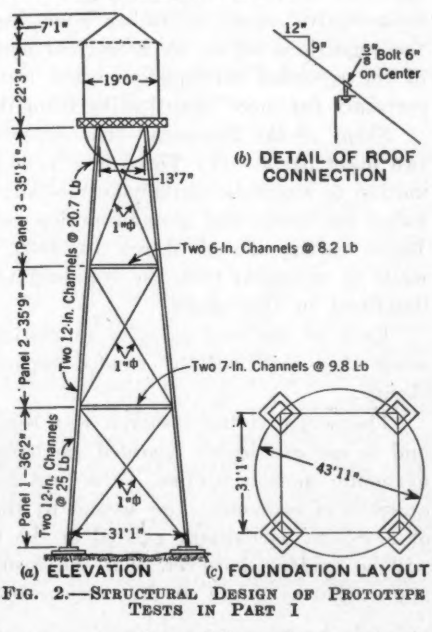

FIG. 2.—STRUCTURAL DESIGN OF PROTOTYPE TESTS IN PART I

It is to be noticed in Fig. 1 that a compound pendulum (not a simple pendulum) represents the water and the tank. This feature corresponds to the restraining action of the tank walls upon the water mass which forces part of the water to move with the tank as it vibrates. A simple pendulum would correspond approximately to a shallow, saucer-shaped tank of water, in which there is little restraint on the lateral motion of the contents.

The problem of determining the earthquake resistance of elevated water-tanks can be solved mathematically in a qualitative way only, not because the mathematical process is unknown, but because of the tremendous work involved in obtaining results of practical value to engineers. Those who have attempted to calculate the effects of irregular transient disturbances on vibrating systems will recognize at once the advantages of the experimental approach to the earthquake problem. The model method, to be sure, is not the only practical way to avoid mathematical difficulties, but, at present, it appears to be the most convenient one. The greatest advantage of the model method lies in its directness, with the attendant simplicity in interpretation of results.

It will be seen that a system is involved which is dynamically complex in itself; it is subjected to ground motions of a transient and complex nature. The problem is to obtain information which has engineering significance in the sense of being truly quantitative rather than merely numerically precise. That there can be a tremendous difference between precision and true quantitativeness is a fact easily overlooked in research. Thus, the results obtained in the simple harmonic motion studies described herein can be regarded as reasonably precise, but from the engineering standpoint their quantitative aspect is rather poor, because they do not directly represent the conditions set up by actual earthquakes. On the other hand, the results of the recorded earthquake-motion tests, although probably less precise, are certainly far more quantitative from the engineering standpoint.

Scope of the Research.—The experimental investigation was divided into two main parts: (1) Tests of a 1:46.5 scale model, using simple harmonic motion to simulate earthquake motion; and (2) tests of a 1:25 scale model, using the horizontal ground-motion components recorded during the Long Beach earthquake of March 10, 1933. In each part, the same model was made to represent both the standard type of construction and the new type described in this paper.

Each of the two parts is so distinctly different in technique from the other that each will be treated separately in this paper for the sake of clarity.

The scope of the research was limited to one particular class of tanks and to one particular recorded earthquake motion in addition to the simple harmonic motion studies. The first restriction was necessitated by considerations of economy; the second by the fact that only one complete record of a violent earthquake existed at the time the research was conducted. It will be evident, however, that the results are not so narrowly restricted in application as might at first appear to be the case.

PART I.—EXPERIMENTS IN SIMPLE HARMONIC MOTION

Standard Design; Model Scale, 1:46.5

Considerations Relative to Building and Testing the Model.—A standard 60 000-gal hemi-spherical bottom tank on a 100-ft tower was chosen as the first object of study. It was situated about 33 miles from the epicenter of

the Long Beach earthquake (March 10, 1933), and about $6\frac{1}{2}$ miles from the point where the earthquake motion used in this research was recorded. Several bracing rods in the top panel were permanently stretched and one rod was broken during the earthquake. The general design of this structure is shown in Fig. 2. The results obtained therefrom, although probably typical of a fairly wide variety of elevated tanks, cannot be applied indiscriminately to other designs. However, they do point to certain general important conclusions as to the inadequacy of present design methods.

The behavior of a water-tank supported on a steel tower is controlled by two kinds of physical forces: (1) Gravity (or weight) forces, which affect the motion of the water within the tank; and (2) elastic forces produced by the tower, which affect the motion of the tank as a whole. These two kinds of forces interact in a complex manner when the tower is subjected to vibratory motions. Therefore, in constructing a model of such a system, it is necessary to maintain the same relation between these two physical forces in the model as exists in the prototype if true similarity is to be attained; that is, gravity forces and elastic forces must be scaled down in the same proportion. This condition precludes the possibility of using a simple geometrically scaled model. It becomes necessary to adopt a common artifice of model technique—scale distortion—to preserve similarity. This may be accomplished by using different scales for the tower and the tank; but there is a simpler and more practical solution.

As far as the interaction between the tower and the tank is concerned, it is easily seen that the tower could be replaced by any other elastic system capable of producing the same horizontal deflection per unit horizontal force applied at the tank, without affecting the behavior of the tank during an earthquake. In other words, the tower merely provides an elastic restoring force proportional to the deflections of the tank, tending to restore it to its position of statical equilibrium. In building a model, it is advantageous to make use of this fact in order to avoid a costly tower model which might prove to be of doubtful accuracy. Accordingly, it was decided to support the model tank on two flat steel bars acting as simple vertical cantilevers, the length of the bars being made adjustable in the laboratory to permit an accurate setting of the stiffness. A symmetrical four-post tower, being equally stiff in all directions, can be simulated in the model by making the supporting bars of the proper stiffness in one direction only and then testing the model in that direction. In this manner the model "tower" can be made strong enough to hold the tank weight safely, and at the same time flexible enough to permit very large deflections without danger of failure.

It is not intended, of course, to determine the ultimate—or failing—strength of the prototype, because the engineer is interested primarily in building structures so that they will not be stressed above the elastic limit. The results of a model study of this kind, therefore, must be interpreted as valid for the prototype to the point where its elastic limit is reached. It is neither practical nor very useful to attempt to duplicate the ultimate

strength conditions in a model, because the range of experimentation would thus be greatly curtailed indeed unless one were willing to build a great many models, each of which would have to be discarded as soon as its elastic limit was exceeded.

The point next in importance to be considered is the choice of scale for the model. There is no simple rule for the determination of what constitutes the ideal scale. The upper limit of model size is fixed largely by considerations of laboratory convenience, expense, and capacity of the available testing equipment. In the lower limit the choice of scale is restricted by the difficulties in recording the minute motions accurately and also in controlling the testing conditions. Other difficulties with very small scale models are brought about by the effect of surface tension and viscosity in the liquid which tends to destroy the similarity, and by the attendant speeding up of the model motions which renders accurate recording and timing more difficult. Traffic vibrations also are likely to become troublesome when very small scales are used.

After considerable study it appeared that a scale of about 1 to 50 would be satisfactory. This proved to be the case. The model of the tank itself was made of No. 26 gage, sheet aluminum, the bottom and roof being spun

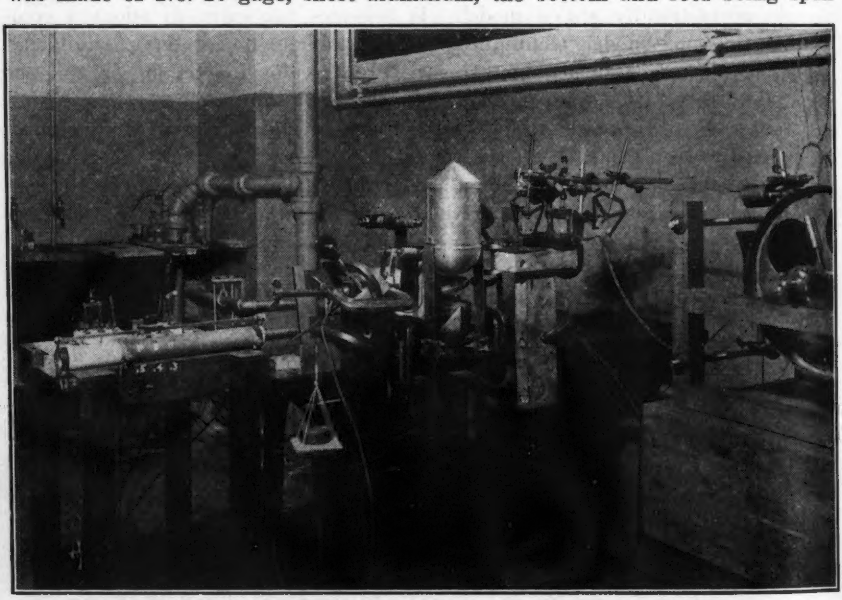

FIG. 3.—MODEL ON FIXED TABLE, SHOWING ARRANGEMENTS FOR ADJUSTING STIFFNESS AND MAKING FREE VIBRATION TESTS

and the joints rolled together. The resulting tank model gave the proper ratio between weight of shell and weight of water, which is a necessary condition for similarity.

The complete model mounted on a stationary table for stiffness adjustment and free vibration tests is shown in Fig. 3. It will be seen that the

legs are clamped at the base so that the stiffness of the model tower is easily adjusted to the desired value. The method of determining the stiffness is also shown in Fig. 3. A known force is applied horizontally at the center of gravity of the tank by means of a weight-and-pulley arrangement, the resulting deflection of the tank being measured by a micrometer microscope reading on a fine scratch on an index attached to the tank.

The tank was attached to the tower legs by means of spring hinges soldered to the top of the legs and to the bracket on the tank. Each hinge consisted of a strip of spring steel, 0.003 in. thick, having an unsupported length of about 0.01 in. This type of hinge is practically frictionless and, at the same time, remarkably rugged. It exhibited no deterioration during the entire set of tests. The tank bracket was riveted to the shell.

It should be remarked that in determining the stiffness of the model tower it is necessary to have the proper quantity of water, or equivalent weight, in the model tank, because the vertical load on the legs has an effect upon their stiffness which needs to be considered. The following relations between model and prototype are easily deduced from model theory[3]: If the length scale $= \lambda$, the time scale $= \sqrt{\lambda}$, and the stiffness scale for the tower $= \lambda^2$. For the actual model the length scale, λ, was 46.5. This gives a time scale of 6.82; that is, a duration of 1 sec in the model corresponds to 6.82 sec in the prototype. The following list of weights, in pounds, shows the relations between the laboratory model and the full-sized tank:

	Weight in pounds
Weight of water in prototype tank (3 in. from top)	503 000
Weight of empty tank, balcony, etc.	31 000
Equivalent tower weight concentrated at center of gravity of tank	11 000
Total concentrated weight at center of gravity of tank	545 000
Weight of water in model tank	5.00
Weight of empty tank	0.24
Equivalent tower weight constructed at center of gravity of tank	0.11
Attachments	0.04
Total concentrated weight at center of gravity of tank. (The model weight corresponds with the prototype weight; thus: $5.39 \times (46.5)^3 = 543\ 000$ lb)	5.39
Stiffness of prototype tower (calculated), in pounds per inch of deflection at center of gravity of tank.	16 000
Stiffness of model tower (as adjusted in laboratory), in pounds per inch of deflection at center of gravity of tank; and $7.37 \times (46.5)^2 = 15\ 940$ lb per in.	7.37

[3] *Civil Engineering*, August, 1935, pp. 457–458.

The stiffness of the prototype tower was computed by applying a horizontal force of 1 000 lb at the center of gravity of the tank to one bent of the tower and calculating the resulting deflection of the tank (0.125 in.). The corresponding angular tilt of the tank was also computed and was found to be only 8 sec under this load; consequently, it is obvious that the rotation need not be considered in building the model, which further simplifies the laboratory work by permitting the use of vertical instead of battered legs.

The foregoing scale factors establish the magnitude and period of the artificial earthquake motion to be applied to the model in order to simulate a given earthquake. For instance, an earthquake in Nature, having a period of 1 sec and an amplitude of 2 in., would be simulated in the model by a motion having a period of $\frac{1}{6.82}$ sec. and an amplitude of $\frac{2}{46.5}$ in.

The shaking-table was loaded with a dead weight of approximately 300 lb, so that the model weight was less than one-fifteenth of the dead weight of the table. This is an important consideration because, if this ratio is not made large, the reactions of the model may seriously affect the table motion. Of course, in the region of resonance this effect is unavoidable, but during the early stages of the motion the effect of the model on the table motion will be quite small, and, after all, it is the early part of the motion that is of greatest interest.

The interpretation of the model results in terms of the full-sized structure is quite simple. The model is so constructed that its deflections are related to those of the prototype according to the length and time scales. Therefore, the deflections that would exist at any instant in the full-sized structure may be determined easily. Having once computed the stresses in the tower, corresponding to a known deflection, the determination of the magnitude of the stresses at any given instant is a matter of simple arithmetic. It is obvious, then, that a record of the motion of the model tank relative to its base is sufficient for the determination of both the stresses and deflections that would be produced in the prototype, as well as the time at which they would occur.

Artificial Earthquake Motion.—In this part of the research an earthquake motion of cosine form was adopted, that being the most convenient to work with in the writer's laboratory. When the table is vibrating by itself, the shaking-table motion is so slightly modified by damping that, over a range of 15 or 20 cycles, the attenuation is quite negligible. When a model is placed upon the table, the record of the table motion appears as a damped cosine wave. This is due to the absorption of part of the energy of the table by the model.

The range of periods chosen for these tests covers the generally discussed band of earthquake frequencies, although there is considerable yet to be learned about the upper and lower limits of earthquake periods. Apparently, the commonly accepted "destructive periods" of approximately 0.75 sec to 1.5 sec, are based largely on the destruction to buildings, the natural periods

of which often fall close to that range. It is not to be assumed, however, that severe ground motions of much longer periods do not exist during severe earthquakes; and it is certain that there are much shorter periods present.

Notation.—The symbols used throughout this paper are defined where they first appear. An effort has been made to conform as nearly as practicable with "Symbols for Mechanics, Structural Engineering, and Testing Materials",[8] compiled by a committee of the American Standards Association, with Society representation, and approved by the Association in 1932.

Functions of the "Dummy Tank".—It was considered to be of interest to compare the behavior of the actual water-tank with that of an equivalent solid mass in order to determine the effect of the water on the motions. Consequently, a small inverted pendulum was constructed and its period adjusted to the proper value. This pendulum is referred to as the "dummy tank" because it behaves in exactly the same manner as the actual tank if the water was replaced by a solid dummy mass of equal weight. In other words, the motions of the dummy tank represent the effects of earthquakes upon a water-tank which is frozen solid.

The dummy tank was made to serve another important purpose at the same time, namely, that of controlling the accuracy of the laboratory work. The calculation of the theoretical motion of the dummy pendulum being a relatively simple matter, it is possible to check the correctness of the work at any time. Since the dummy pendulum and the actual tank were always tested simultaneously, it is safe to assume that if the theoretical dummy motion checks the recorded motion satisfactorily, the motions of the actual tank must also be correct.

The proper natural period, P, for the dummy pendulum is found as follows:

$$P = 2\pi\sqrt{\frac{W}{g\,k}} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (1)$$

in which W = weight of tank plus contents (assumed to be solid); g = acceleration due to gravity; and k = spring factor of tower (restoring force per unit deflection). From the foregoing lists of weights and scale relations: $P = 2\pi\sqrt{\dfrac{545\,000}{32.2 \times 12 \times 16\,000}} = 1.863$ sec for the prototype. Then, since the time scale is $\sqrt{\lambda} = 6.82$, $P_m = 1.863 \div 6.82 = 0.273$ sec for the model. The correctness of the model-scale relations is easily verified by calculating P_m, using the actual model constants; thus: $P_m = 2\pi\sqrt{\dfrac{5.39}{32.2 \times 12 \times 7.37}}$ $= 0.273$ sec. (The dummy pendulum was adjusted by trial to a period of 0.271 sec, within 1% of the theoretical.)

It is easily proved that the dynamic response of a pendulum of this type is dependent only upon its natural period, and independent of its physical dimensions. Therefore, it was not necessary to make the mass and spring

[8] A S A—Z10 a—1932.

factor of the dummy identical with those of the tank model. The dummy pendulum was made by attaching a small mass (about 0.5 lb) at the top of a vertical flat steel bar. This type of small pendulum was adopted in order to reduce the total effect of the models upon the table motion to a minimum.

Laboratory Procedure.—The laboratory work was divided into two main parts—free vibration tests and shaking-table tests. In the free vibration tests, the model was mounted on a rigid stationary table and the tests were made by suddenly releasing the model from a pulled-back position. The resulting motions of the tank were recorded photographically. Fig. 3 shows the general arrangement used for recording the free vibrations. The pull-back and release were accomplished by means of standard link fuses. One end of the fuse was clamped to a small bracket (see at the top of the left tower leg); the other end was clamped to a bracket attached to the table, the latter bracket being arranged with screw adjustment to permit varying the amount of pull-back at will. The shield over the fuse kept the light of the flash from striking the recording paper. A 110-volt circuit, shorted across the fuse, gave a very fast release.

At the right of the tank is a swiveled mirror and lens arranged to trace the model motions on the recording drum, the source of light being just above the front of the drum (extreme right of Fig. 3). Immediately to the right of the recording mirror is another lens for recording the time intervals which are marked by the passage of a periodic electric spark. The spark-gap is shielded, but the terminals where the leads connect may be seen in Fig. 3.

The shaking-table motions were produced in a manner similar to that of the free vibrations, except that it was necessary to use a short length of resistance wire, instead of a link fuse, in order to deflect the stiff springs of the table. A special form of wire link was developed for this purpose, which proved to disintegrate very rapidly under a 110-volt current with no detectable effect upon the table motion. This was accomplished by reducing the length of the wire to be burned to about $\frac{1}{8}$ in.

The set-up for the shaking-table test has been amply described elsewhere[4]. The table itself was suspended entirely by piano wires and actuated by springs. Lateral guy wires leading to the floor prevented any tendency of the table to twist due to unbalanced forces.

Three mirror-and-lens systems, which received light from three illuminated slits beneath the recording drum were arranged so that one recorded the motions of the shaking-table relative to the floor, while the second recorded the motions of the tank relative to the shaking-table (the deflections); and the third recorded the deflections of the dummy pendulum relative to the table. A fourth lens system recorded the time marks. The records were made on bromide paper.

Fig. 4 represents a part of a typical record. The axes of the three motions were separated purposely for the sake of ease in reading the records.

[4] *Civil Engineering*, August, 1935, p. 458, Fig. 7.

Just before releasing the table, the drum was revolved once with the lights on, and the timer off, to provide base lines for measuring the deflections. The entire record (39 in. long) being made in about 5 sec, it was necessary to time the recording procedure carefully to prevent awkward overlapping. The switchboard which controlled the table release, lights, timer, and recording drum, is seen in Fig. 3.

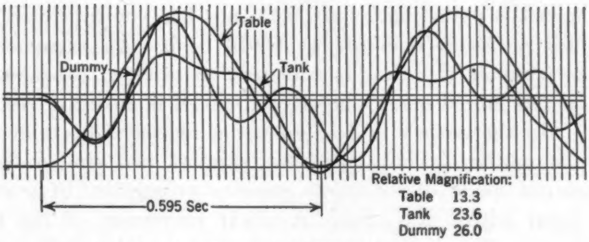

FIG. 4.—PART OF A TYPICAL RECORD

Free Vibration Characteristics.—As the pull-back amplitude is varied, the resulting free vibrations prove to be very uniform in character. Analysis of the records shows the system to be remarkably linear, in view of the complex action of the water inside the tank. Fig. 5 is a plot showing various successive maximum amplitudes of free vibration, expressed in terms of the percentage of the original pull-back before releasing. (In this and all succeeding diagrams all data are given in terms of the full-sized tank in order to avoid confusion as to scale relations.) In a perfectly linear system such as one made up of springs and weights, these curves would all be horizontal straight lines. In the water-tank the curves are quite straight, but all show more or less deviation from the horizontal. However, the percentage deviation within any reasonable working deflection of the tank is

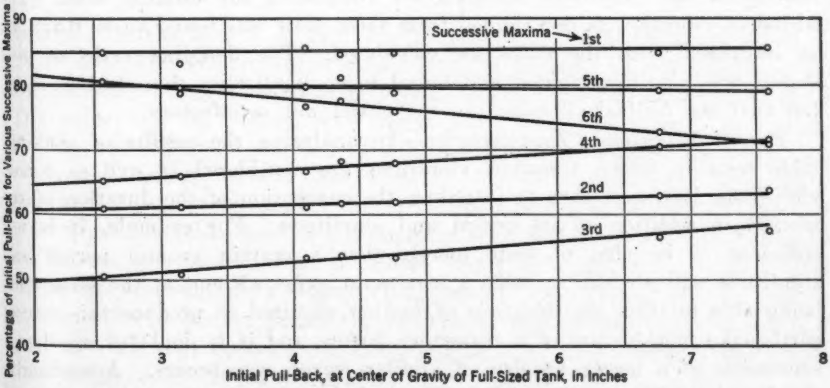

FIG. 5.—FREE VIBRATION BEHAVIOR OF TANK WHEN SUDDENLY RELEASED FROM PULLED-BACK POSITION

quite small. On the basis of this observation one might expect that the system would exhibit nearly linear characteristics in the shaking-table tests, and this was found to be the case.

The curves in Fig. 5 are not extended below deflections of 2 in., because it was found that as the deflections became quite small the characteristics of

the pull-back fuse began to effect the resulting free vibrations sometimes by as much as 5 or 6 per cent. It is easily seen why this can be true when it is realized that, corresponding to a pull-back in Nature of 1 in., the model is pulled back only 0.02 in. The energy stored in the model tower for this pull-back amounts to 0.0016 in-lb. An extremely short lag during the melting of the fuse is sufficient to produce considerable percentage change in this stored energy before the model is entirely free.

In the records of free vibration two periods stand out distinctly, although the motion is not quite the sum of two simple harmonic motions. The slower mode of vibration consists principally of a swinging motion of the water within the tank. A slight movement of the tower accompanies this mode. The second and faster mode consists of a swaying back and forth of the tank as a whole, while a large part of the water moves with the tank. This mode resembles the action of a simple inverted pendulum. These two periods were estimated from the records to be 0.39 sec and 0.24 sec, respectively, for the model. (The corresponding periods for the full-sized structure are 2.7 and 1.6 sec.)

It is interesting to compare these periods with that of the dummy tank model which has a period of 0.27 sec. The change from 0.27 to 0.24 sec and the addition of the secondary period of 0.39 sec are created entirely by the freedom of the water to move relative to the tank.

In order to verify the accuracy of the model and laboratory work, free vibration tests were also made with the tank filled with a mixture of sand and sawdust, of volume and weight equal to that of the water (5.00 lb). This mixture was solid enough to act as an inert mass in the tank so that it duplicated the conditions assumed for computing the dummy tank. The period of vibration as determined from these tests was found to be 0.272 sec as compared with the 0.273 sec calculated. The damping ratio of this system was also investigated and found to be small (less than 1.015), showing that the method of mounting the model was satisfactory.

Forced Vibration Characteristics.—In analyzing the results of shaking-table tests in which transient vibrations are considered as well as forced vibrations, it is necessary to introduce the conception of the duration of the motion in addition to its period and amplitude. For example, it is not sufficient to be able to state merely that a certain ground period and amplitude will produce so large a maximum stress without at the same time being able to state the duration of motion required to produce this stress; earthquake motions are of a transitory nature and it is doubtful whether a succession of a larger number of similar waves ever occurs. Accordingly, the data have been studied in the light of the maximum stresses (or deflections) that occur up to various periods of time, the measure of time adopted being the number of ground cycles elapsed since the start of the motion. This arrangement of data gives a clearer view of the matter than one in which time intervals are expressed in seconds, because it is the number of ground vibrations rather than the absolute duration of time that is of greatest interest.

Fig. 6 presents the results of the shaking-table tests in a condensed form. The maximum amplitudes of the tank and dummy are shown as functions of the ground period, holding the maximum acceleration of the

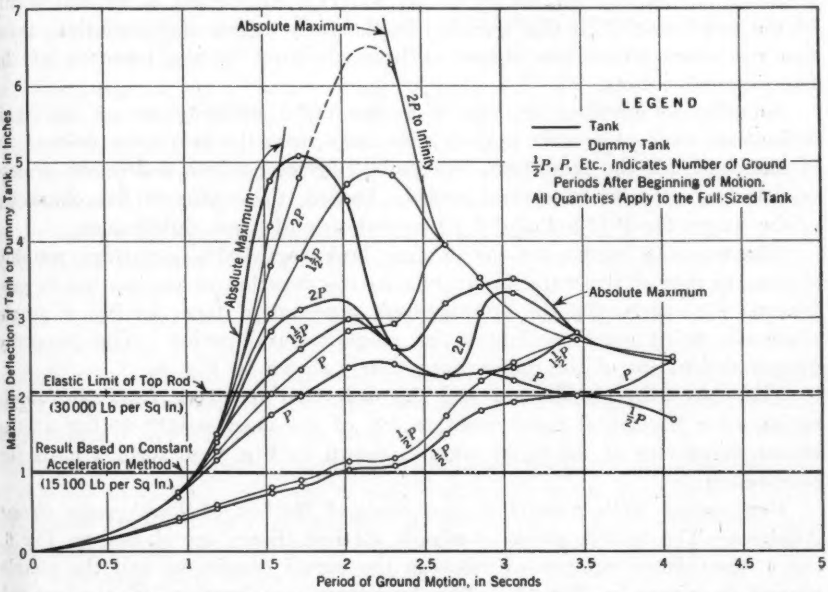

FIG. 6.—RESULTS OF SHAKING-TABLE (MAXIMUM GROUND ACCELERATION = 3% OF GRAVITY; FOR OTHER VALUES USE DIRECT PROPORTION)

ground constant at 3% of that due to gravity. (These data are given in terms of the maximum ground acceleration because many engineers are accustomed to compute earthquake stresses on the basis of acceleration alone. The value, 3% of gravity, was chosen as a base for plotting the results because the structure would have a safety factor of 2 when subjected to a constant horizontal acceleration, 0.03 g; that is, according to the statical method of earthquake design the structure would have a safety factor of 2 for any earthquake having a maximum acceleration of 0.03 g.) Because the system was found to be so nearly linear it is possible to derive other values of acceleration by direct proportion. Of course, this is slightly approximate at certain points on the curves and probably would not hold true for very large accelerations (say, greater than 0.15 g) even if the elastic limit point were indefinitely high.

One of the most outstanding characteristics of these curves is the extreme difference between the form of the early maximum curves and that found in the absolute maximum curves. First considering the actual tank, one notes in the absolute maximum response the appearance of two distinct resonance points. These points correspond to the natural periods observed in the free vibration tests. (The maxima in Fig. 6 do not coincide with the natural periods—2.7 and 1.6 sec—because of the method of plotting the

results. If Fig. 6 is replotted, keeping the ground amplitude constant instead of the maximum acceleration, it will be found that the maxima then appear at the natural periods of the system.) The resonance point corresponding to the natural period of the water is suppressed, a natural result of the non-linearity in this region. In the early stages of the motion, these two resonance points are almost entirely obscured by the presence of the transient vibrations.

Equally outstanding in Fig. 6 is the rapid building up of the tank deflections, even at ground periods well away from the resonance points. It is seen that, for the most part, two ground cycles produce deflections almost as great as a series of several cycles. Indeed, the relatively flat character of the curves for P (1.5 P and 2 P) is obviously of great significance.

The response curves for the dummy tank begin with a pattern roughly similar to that of the water-tank; but, as the duration of the motion is prolonged, the curves for the dummy tank approach a form having a single resonance point corresponding to its single natural period. The generally greater deflections of the dummy are clearly shown in Fig. 6.

The elastic-limit deflection and the deflection resulting from the application of a horizontal force equal to 3% of the dead weight acting at the center of gravity of the tank, are also shown in Fig. 6 to form a basis for comparison.

Comparison with Results of the Statical Method of Earthquake Stress Analysis.—The results given by simple statical theory are shown on Fig 6, but a more direct comparison between the actual conditions and the simple theory is given in Fig. 7. The maximum acceleration of the ground

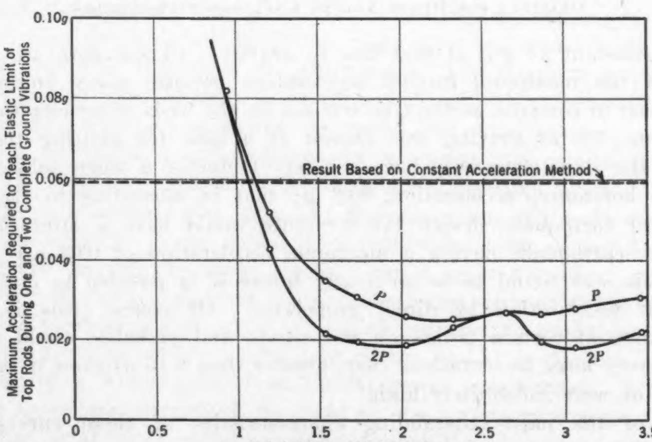

FIG. 7.—COMPARISON WITH SIMPLE THEORY OF DESIGN

(expressed as a percentage of gravity) necessary to produce elastic-limit stresses during one and two ground vibrations is plotted as a function of the ground period. It is obvious that the simple theory based on a constant acceleration fails to fit the true conditions, even as a rough approximation.

The curves cross in the vicinity of the 1-sec ground period, but this is a coincidence rather than a result of the assumptions upon which the simple theory is based. The inadequacy of this type of tower is shown strikingly in Figs. 6 and 7.

Conclusions Pertaining to the Standard Design.—The most important conclusion to be drawn from the results of this part of the research concerns the inadequacy of the generally accepted method of designing elevated tank towers against earthquakes. It is clearly shown that the assumption of a static horizontal force proportional to the maximum ground acceleration leads to large errors over practically the entire band of possible earthquake frequencies. If earthquakes did not exhibit periods shorter than 5 or 6 sec, the fixed-force method of analysis could be considered fairly satisfactory for tank design provided a reasonable factor of safety was adopted. Unfortunately, the more violent motions of the ground during an earthquake are known to contain periods considerably shorter It would be possible, of course, to make the simple theory fit elevated tank design fairly well by building the towers of such stiffness that the natural period of the structure would be shortened, say, to 0.25 sec; but with a mass of 500 000 lb elevated approximately 100 ft above the ground, such a design would be extremely expensive. In other words, unless the natural period of the structure can be reduced to at least one-third or one-fourth that of the shortest important earthquake period to be expected, the dynamical effect of the ground motion can not safely be ignored.

It will have been observed from Figs. 6 and 7 that the effects of earthquakes upon the structure are expressed in terms of motions or deflections, the unit stresses produced by these motions being incidental to them and not controlling them. It may be seen that, in any given tank tower, the best effect that can be had from a moderate amount of strengthening is obtained by strengthening those members which are inherently weak until all the members of the structure reach ultimate stresses simultaneously. Then the structure achieves the maximum possible safe deflection; this means that, for the tower studied in this research, an increase in the size of the rods in the upper panels would strengthen the tower dynamically to some extent (since they are the highest stressed rods); but that, paradoxically enough, if all the rods were increased in cross-section by the same percentage, the tower would have the same strength dynamically as it had originally, because the safe deflection would be unchanged.

The conclusion that large tower deflections can be produced in short periods of time over a wide range of earthquake frequencies is obvious from the data of Fig. 6. This conclusion leads further to the consideration that an actual earthquake motion may introduce dangerously large amplitudes even if the condition of resonance or near resonance is entirely lacking.

It is clear that although they indicate in a general way the effect to be expected, the simple harmonic motion tests do not offer a practical means of arriving at quantitative engineering data on the effects of actual earthquake motion. On the basis of the familiar Fourier harmonic analysis,

the simple harmonic test can be regarded as a reliable "measuring stick" for comparing the dynamic behavior of one system with that of another system; but beyond demonstrating the possibility of the process mathematically for certain structures one cannot practically go very far toward converting the results of simple harmonic motion tests in terms of the effect of a given irregular earth motion.

Spring Element Design; Model Scale, 1:46.5

Considerations Relative to Development of Earthquake-Proof Tank Structures.—A study was made of a number of possible types of tank and tower construction, with a view toward finding a type or types suitable for active seismic regions. Some of these types, of course, appear to be fantastic or impractical, whereas others are uneconomical. Among the former may be listed such arrangements as roller supports for the foundations or tanks, various ways of hanging or suspending the tank so as to decrease its vibrations, etc. Among the uneconomical designs are those which seek to give the structure adequate quake-resistance by providing an extremely stiff or rigid tower. The data in Table 1 will serve as an illustration. Thus, it is clear that it is quite uneconomical to attempt

TABLE 1.—Comparative Designs, 60 000-Gallon Tank on a 100-Foot Tower (Distance from Column Base to Balcony = 108 Feet)

Item No.	Design (1)	Number of panels (2)	Weight, in tons, more than Item No. 1 (3)	Natural vibration period, P, in seconds (4)
1........	Standard design*....................	3	0	1.50
2........	Design for a constant acceleration of 0.1g	3	8.25	1.13
3........	Extra heavy design..................	3	10.25	0.99
4........	Extra heavy design..................	4	11.50	0.93

* Diagonal rods, 1 sq. in. in area.

to give very short natural vibration periods to heavy tanks on towers of this height. For capacities of 100 000 gal and more, and for towers of greater height, the situation is very much worse.

If the natural period cannot be reduced to about 0.5 sec, it is doubtful whether the tank is very much more likely to survive a violent earthquake than a standard design built for wind resistance only. The odds will depend upon the nature of the particular ground motion to which it may be subjected and upon the foundation conditions.

The natural conclusion from a prolonged study was that, barring fantastic and impractical designs, the best way to obtain adequate quake-resistance is to provide the tower with a greater range of elasticity; that is, if the tower can be built so that the safe deflections are large enough, there will be no danger of failure. This does not necessarily imply that the tower must be extremely limber or weak, although a certain degree of

limberness is almost always created by increasing the allowable deflection of a structure. This concept, of course, is not a new one, but it has been most often discussed in relation to buildings; and, in this connection, the writer wishes to emphasize that the present discussion is in no way intended to apply to buildings.

There are two general methods of increasing the allowable or elastic deflection range of a structure such as a water-tank tower: (1) By making the tower of some type of open framework in which the forces resisting deflection are produced by bending stresses set up in some or all of the members of the framework; and (2) by artificially increasing the allowable direct-stress deformations of some or all of the members of a statically determinate tower. Of course, combined systems will also suffice.

Method (1).—The first method has one serious drawback—if the tower is made with sufficient deflectibility for quake resistance, the deflections due to strong winds are likely to be undesirably large and the tank will sway considerably with every light wind. This difficulty can be avoided by adding a system of diagonal bracing to the tower, the bracing being of such strength that it will safely stand ordinary wind force but will break or collapse when a serious earthquake occurs, leaving the tank free to sway as far as need be during the quake, and yet not endangering the structure as a whole (if it is made inherently stable).

Difficulties arise in the design of a fool-proof collapsible bracing system, since a bracing failure on one side only or on one panel only would probably be disastrous. Furthermore, questions arise as to the advisability of taking chances on hurricane or near-hurricane winds which would break the bracing and leave nothing to resist them but the bending action of the frame (which, for towers about 100 ft high, is rather small if the design is economical). The replacement of bracing (or of breakable links in the bracing) after an earthquake does not present any serious objection. All in all, it may be stated that this method is at least worthy of serious consideration and perhaps it would be well worth while to study the behavior of models representing various towers of this general type.

Method (2).—The second method can best be explained by reference to Fig. 8 which illustrates schematically a device developed during this research. If spring elements such as those in Figs. 8(*b*) and 8(*e*) are introduced at Points *S* into the bracing of the tower (Fig. 8(*a*)), it will be seen that the allowable elastic deflection of the tower can be increased at will within very wide limits. Furthermore, it will be noticed that the spring elements are provided with a means of applying arbitrary initial stresses to the springs, so that up to the limit of the initial stresses they are perfectly rigid, thus preventing undesirable sway of the tank from ordinary wind forces. When an earthquake of serious magnitude occurs, however, the initial stress is quickly overcome and the elements act as simple springs to the point at which the coils close up tight, when they again become perfectly rigid.

It is evident that this method of construction has the advantage over the first method in that here extremely high winds or hurricanes will never

lack adequate tower resistance as long as the main members of the tower are large enough to take the stresses. Furthermore, a slight dissymmetry will not be disastrous, as compared with the effect of breaking the bracing on one side of the tower.

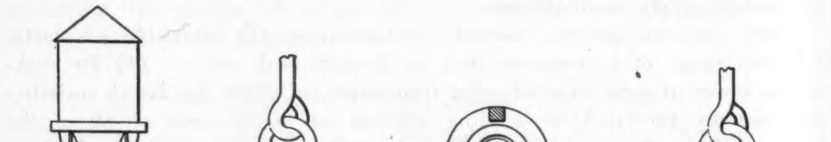

FIG. 8.—SPRING ELEMENT CONSTRUCTION (SCHEMATIC)

There are so many possible types and combinations of spring elements that no attempt will be made to catalog them all. One special type that is of interest, however, is shown in Fig. 9, in which a damping device has been added to the spring element as a means of decreasing the amplitudes of vibration. It is surprising to find how little damping force is needed to produce a very beneficial effect in the behavior of this type of construction.

Description of Model Technique.—In dealing with an entirely new type of construction, where there are no definite data at hand on which to base design, it is desirable to investigate a number of possible designs under similar conditions. To build a series of models, each representing some variation in design, would obviously be a very expensive and difficult procedure. Therefore, it was decided to conduct this preliminary study by means of a single model which could be adapted to represent a number of designs and, at the same time, would entail a minimum of time and expense.

In order to understand how this was accomplished, it is first necessary to examine the statical, or load-deflection, characteristics of a structure

equipped with spring elements. Fig. 10 shows the typical theoretical load-deflection curve for the new style of tower. This curve is obviously ideal-

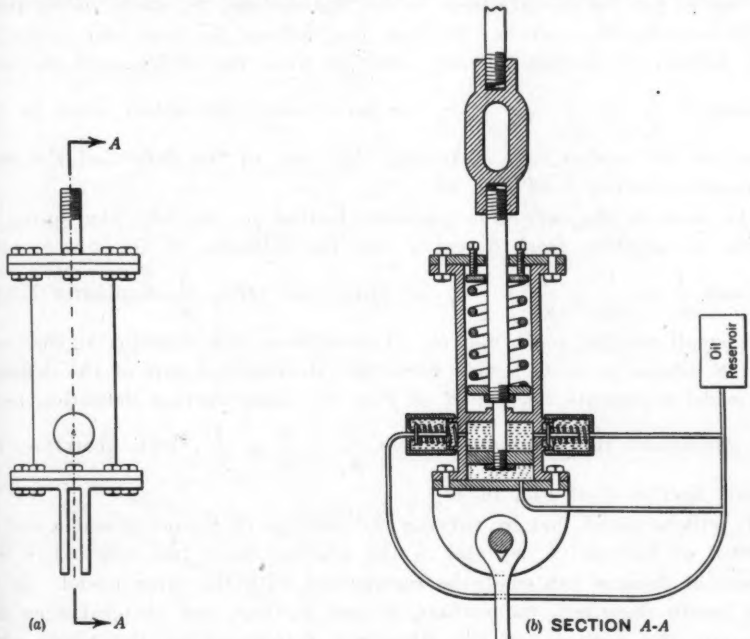

FIG. 9.—SPRING ELEMENT WITH SPECIAL DAMPING DEVICE (SCHEMATIC)

ized; in practice, the sharp breaks will always be more or less rounded off due to unavoidable inaccuracies in building the structure and in adjusting the rod tensions.

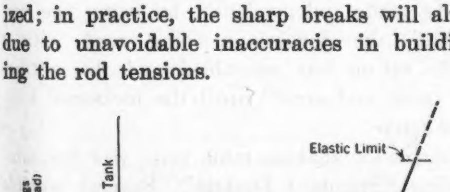

FIG. 10.—LOAD-DEFLECTION CHARACTERISTICS OF A TANK TOWER EQUIPPED WITH SPRING ELEMENTS

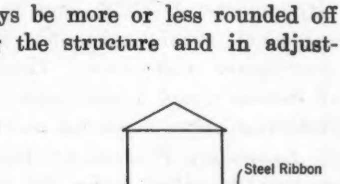

FIG. 11.—DIAGRAM OF MODEL USED FOR REPRESENTING VARIOUS SPRING ELEMENT DESIGNS

Now if it is sufficient to treat the structure as a two-dimensional problem (as in the previous tests), there is a very simple method of building

its dynamical equivalent. Fig. 11 shows how the model was constructed by simple changes in the original model of the standard design. Screws a are set to put an initial stress in the flat springs, S_1, when the system is in its equilibrium position. If, then, one deflects the tank over to the left, say, Ribbon r_L buckles at once, and, at first, the stiffness of the tower system is $\frac{1}{S_3} + \frac{1}{S_3} + \frac{1}{S_2}$ to the point where the initial stress in S_1 is equal to the tension in r_R. During this part of the deflection the model represents Section A of Fig. 10.

As soon as the deflection proceeds farther to the left, the spring, S_1, begins to separate from Screw a and the stiffness of the tower system becomes $\frac{1}{S_3} + \frac{1}{S_3} + \frac{1}{S_2} + \frac{1}{S_1}$, in which the term, $\frac{1}{S_1}$ dominates because S_1 is small relative to S_2 and S_3. The stiffness now remains at this value until S_1 comes in contact with Screw b. During this part of the deflection the model represents Section B of Fig. 10. Any further deflection to the left encounters the original stiffness, $\frac{1}{S_3} + \frac{1}{S_3} + \frac{1}{S_2}$, and, therefore, represents Section C of Fig. 10.

It will be noted that by varying the settings of Screws a and b and the stiffness of Springs S_1, S_2, and S_3 (by altering their free lengths), a wide variety of designs can easily be represented with the same model. In the tests herein described, the springs, S_2 and S_3, were not changed since they represent the stiffness of the structural framework of the tower which would not change for the various spring-element designs considered. The adjustments of S_1, S_2, and S_3 were made independently by using the conventional weight-and-pulley loading, the deflections being read with a micrometer microscope. Then, the set-up was assembled and the settings of Screws a and b were made by "trial and error" until the measured load-deflection curve fitted the assumed curve.

Laboratory Procedure.—The routine of shaking-table tests was the same as that described under the heading "Standard Design." Several possible designs were chosen for study, and each design was put through a series of simple harmonic-motion tests in which both period and amplitude were varied. For each design, curves were drawn showing the maximum deflection of the tower as a function of ground amplitude for a given ground-motion period, from which could be determined the largest simple harmonic ground amplitude which the structure could safely withstand at that period. Fig. 12(a) shows a characteristic plot for one of the designs investigated. For comparison is shown the corresponding curve for the standard design described in Table 1 and supporting text.

Forced Vibration Characteristics.—A study of Figs. 10 and 12 (a) will help to make clear the relation between statical non-linearity and dynamical non-linearity. The tank deflections produced by a ground motion of cosine form are not proportional to the amplitude when the

period is held constant, as was the case with the simple standard structure. This phenomenon is due to the fact that the natural vibration period of the structure changes as the amplitude of its vibration varies. Thus, from

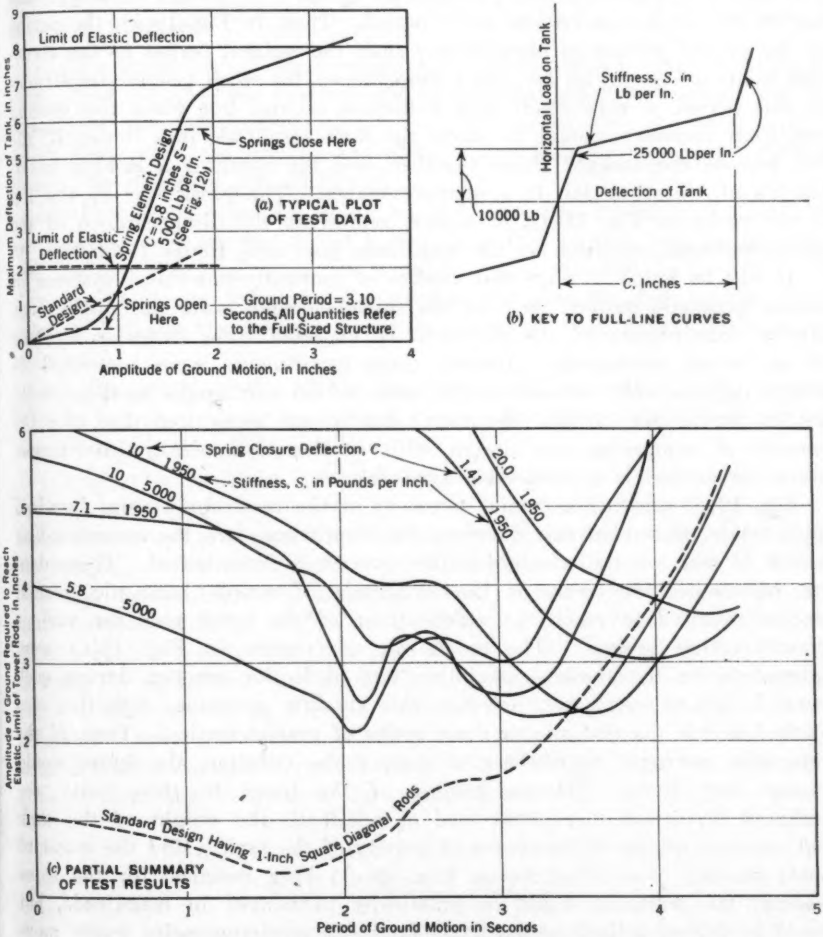

FIG. 12.—EARTHQUAKE RESISTANCE OF SPRING ELEMENT DESIGNS COMPARED WITH THAT OF STANDARD DESIGN

Fig. 10, it is evident that over the range, A, the natural period is constant and is equal to that of the same structure without spring elements. As soon as the amplitude of vibration passes over to the range, B, however, the natural period becomes progressively longer with increasing amplitude because the effective stiffness (that is, the ratio of the horizontal load to the corresponding deflection) becomes progressively less. This state of affairs continues until the amplitude of vibration begins to enter the range, C, when further increase in amplitude will result in progressive

shortening of the period that the structure had at the end of the range, B. However far the deflection may go in the C range, the natural period can never be as short as the period in the A range.

In such a system, then, it is obvious that the amplitude of the ground motion can be just as critical as its period. Thus, in Fig. 12(a), the period of the ground motion is much longer than the natural period of the structure in Range A of Fig. 10. As a consequence, for small ground amplitudes of this period, a very small tank deflection occurs; but when the ground amplitude increases enough to throw the tank amplitude into Range B, the two periods are brought closer together, and the result is a sudden building up of vibration due to a quasi-resonance. The break-over at the top of the curve in Fig. 12(a), of course, represents the disappearance of the quasi-resonance condition as the amplitude goes into Range C of Fig. 10.

It will be noted at once that studies of markedly non-linear systems in simple harmonic motion, such as the spring-element tower designs, cannot give a clear picture of the effects to be expected from irregular motions of an actual earthquake. Indeed, these experiments were restricted to simple motions only because at that time actual earthquake motions could not be reproduced in this laboratory; but it was recognized that, for the purpose of comparing one design with another dynamically, the simple harmonic motion is a serviceable yardstick.

Fig. 12(c) presents a partial summary of the results of several hundred tests. Also, shown on this diagram, for comparison, are the corresponding results of tests on the standard design previously investigated. These data are represented in terms of the amplitude of simple harmonic ground motion required to reach the elastic limit of the tower rods for various ground-motion periods. The points on the curves in Fig. 12(c) were determined by the absolute maximum tank deflection reached during each test. In almost every test, however, this absolute maximum deflection was reached within the first two or three cycles of ground motion. Thus, if the data were restricted to motions of three cycles duration, the figure would change very little. The rod-bracing of the tower in these tests was designed for a safe horizontal load of one-tenth the weight of the tank and contents acting at the center of gravity of the tank. Had the standard tower bracing (see dotted curve, Fig. 12(c)) been designed for the same loading, the ordinates would be practically unchanged in magnitude, but would be shifted a little to the left. Thus, the minimum point would move from a period of 1.6 sec to about 1.3 sec, with no change in magnitude.

The superiority of the spring element design over what is generally thought of as the principal part of the earthquake spectrum is very marked, but for earthquake periods beyond about 3 sec the standard design compares favorably with it.

Some preliminary experiments were made with a frictioning device in order to study the effect of damping. The results indicated that surprisingly small damping forces would suffice to raise the low points on the curves well above the curve for the standard design. These tests were not sufficiently quantitative to warrant extended discussion.

Conclusions Pertaining to Spring Element Design.—These preliminary experiments suggested that the new form of construction has very definite possibilities as a means of overcoming the earthquake hazard for elevated water-tanks. The conclusions to be drawn are quite limited by the very nature of the tests:

(*a*) For earthquakes characterized by short-period vibrations (not more than about 2.5 or 3-sec periods) the simple spring-element design can be considered a satisfactory solution. For such earthquakes the data in Fig. 12(*c*) show that the greater the closure deflection, *C*, the more violent shocks the structure can withstand.

(*b*) For earthquakes in which moderately large long-period vibrations are present (say, between 3 and 6 sec) dangerous stresses may be reached; and, without damping, the spring-equipped structure may be no better off than one of standard design. For such earthquakes the data in Fig 12(*c*) seem to indicate that closure deflections of more than 7 or 8 in. may be undesirable.

(*c*) The presence of a slight degree of damping action would greatly help the structure in the long-period range without appreciably reducing its effectiveness in the short-period range.

(*d*) In order to obtain design data for engineering purposes, tests should be made with actual earthquake motions. The model preferably should be three-dimensional and large enough to permit accurate determination of the damping forces required for practical design. (In the model of 1:46.5 scale the measurement of damping forces is made difficult by the fact that the force-scale is $(46.5)^3$, or 100 500; that is, a force of 1 000 lb in Nature corresponds to less than $\frac{1}{6}$ oz in the model. Although there is no difficulty in measuring very small elastic forces in a frictionless structure such as that shown in Fig. 11, it is difficult to produce and measure frictional forces below the level of 0.5 oz with reasonable accuracy. No such difficulty appears in measurements of deflections, however, since the scale of deflection is equal to the scale of the model).

PART II.—EXPERIMENTS WITH ACTUAL EARTHQUAKE MOTION

Model Characteristics and Testing Procedure

Introductory—Four important questions were left unanswered by the research discussed in Part I:

(*a*) How will the new construction behave in three dimensions?

(*b*) What will be the effect of actual irregular earthquake motions as compared with the simple harmonic motion previously used?

(*c*) How much damping is needed to avoid excessive stresses?

(*d*) How does the proposed structure compare with the standard type of structure in dynamic behavior when subjected to actual earthquake motions.

Before further model studies could be directed profitably toward answering these questions, it was necessary to construct a shaking-machine capable of reproducing actual earthquake motions. The design of this machine[5] was begun in the fall of 1934. Construction was completed in the summer of 1935; and by September, 1935, the operation of the machine was considered quite satisfactory for conducting the research.

The research described in Part II should be regarded as only a part of the program of tank-tower investigation that was originally planned. Limited funds made necessary the curtailment of the planned program, with the result that the data are not nearly so extensive as could be desired. Sufficient engineering data have been obtained, however, to make possible for the first time rational design of a fairly wide range of sizes of elevated water-tank structures for seismic regions.

It is not intended to suggest that the spring element design is the only practical solution to the problem. It is the only solution which the writer has (to date) considered sufficiently practical for actual construction.

The Model.—A three-dimensional model was constructed on a scale of 1:25 to represent a 60 000-gal tank on a 100-ft tower. Spring elements were built into the model to simulate the new type of construction to be investigated.

The basic theory applying to models of water-tank structures was discussed[6] by the writer in 1935, who then showed that perfect geometrical scaling will result in dissimilarity unless the model tower is built of a material having a modulus of elasticity of $\frac{\rho_m}{\rho\lambda}$ times that of the steel in the prototype (ρ and ρ_m being the densities of the liquids in the prototype and model tanks, respectively; and λ is the scale of the model). This means that if mercury was used as the liquid in the model, and brass (E = 13 800 000 lb per sq in.) for the structural parts and tank shell, then a geometrically scaled model with $\lambda = 29$ would be entirely satisfactory (with a minor correction for the density of the brass).

A consideration of the hazards and difficulties involved in the use of a mercury-filled model led to the decision that this procedure should be avoided. This was done by means of a scale distortion analogous to that applied in earlier studies. The expense of building an accurately made geometrical-scale model of a water-tank and tower would have been very great, and the problem of simulating riveted connections exactly to small scale has not been solved. Fortunately, in tank towers of conventional design, the structure is so nearly statically determinate that it makes little difference whether the joints are rigid or pinned, as far as the dynamical behavior of the structure is concerned. Usually, they are somewhere in between, probably being nearer the rigid condition, except where the struts are actually pinned to the columns, as is sometimes the case. The eccen-

[5] *Civil Engineering*, March, 1936; and *Bulletin*, Seismological Soc. of America, July, 1936.

[6] *Civil Engineering*, August, 1935.

tricity of the members coming together at a joint is often of more consequence in affecting the tower stiffness than the degree of rigidity in the joint itself

It was decided to make the joints of the model rigid (the prototype joints were riveted) and to preserve the eccentricities as they are in the actual design chosen for study. In this manner the net tower stiffness can be simulated closely by a simple model.

In order to arrive at the model-member design required for a given over-all length scale, λ, the model theory referred to in Part I is merely extended to the individual parts of the structure.[7] The following relations are easily obtained:

For structural members,

$$\frac{A}{A_m} = \frac{\rho}{\rho_m} \frac{E_m}{E} \lambda^3 \qquad (2)$$

$$\frac{I}{I_m} = \frac{\rho}{\rho_m} \frac{E_m}{E} \lambda^5 \qquad (3)$$

and, for thickness of tank and roof plates,

$$\frac{t}{t_m} = \sqrt[3]{\frac{\rho}{\rho_m} \frac{E_m}{E} \lambda^4} \qquad (4)$$

in which A = area of member in prototype; A_m = area of member in model; t = plate thickness in prototype; t_m = plate thickness in model; I = moment of inertia of prototype member; and I_m = moment of inertia of model member. The thickness scale for the tank and roof plates is correct only if the principal shell distortions are due to the bending of the plates and not to direct stresses. This is obviously the case for tanks of the type studied herein.

It is important to realize that these relations, although resulting in true dynamical similarity, do not result in similarity of unit stresses, from which it is to be concluded that a model built according to these relations properly represents the prototype, in so far as statical and dynamical deflections are concerned, to the point where elastic limit stresses are reached either in the model or in the prototype. Beyond this point true similarity ceases to exist, and the interpretation of the model results becomes a matter of judgment. Once the model deflections are known, it is a simple matter to calculate the corresponding stresses in the prototype as explained in Part I.

To satisfy, simultaneously, both the $\frac{A}{A_m}$ and the $\frac{I}{I_m}$-relations for $\lambda = 25$ (the scale chosen) and $\frac{\rho}{\rho_m} = 1$ (water in the model tank) is a practical impossibility for the sections comprising a standard tank

[7] *Bulletin*, Seismological Soc. of America, July, 1934, pp. 170 to 230.

tower; but there is a simple solution of the problem. If the scale of $\frac{I}{I_m}$ is made correctly, the area scale can be corrected by artificially giving to the members the compliance (force per unit elongation) they would have if the area scale could be maintained. If C and C_m are the compliance of prototype and model, respectively, it can be shown that for the model in question the following relation must be satisfied[7]:

$$\frac{C}{C_m} = \frac{\rho}{\rho_m} \lambda^2 \qquad (5)$$

In the present problem it was found that, when the proper $\frac{I}{I_m}$ -ratio was maintained, the areas of the model sections were invariably much too large. Consequently, the introduction of additional spring action into the members was necessary. Instead of altering the compliance of all the members of the frame, it was found much more convenient to attach appropriate springs to the diagonal rods only in order to correct for the stiffness of the tower as a whole. The reader will observe that the necessary calculations are straightforward, the model springs being chosen so that with a horizontal load acting at the tank, the total deflection in any one panel was made the same as it would have been had all the members in the panel been corrected.

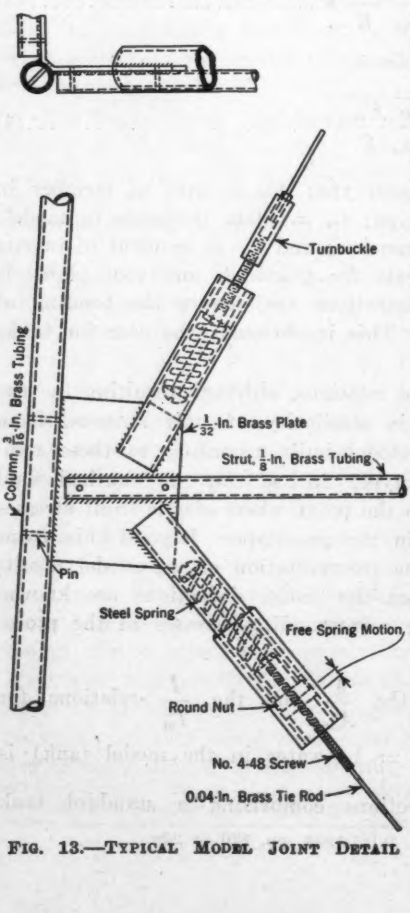

FIG. 13.—TYPICAL MODEL JOINT DETAIL

Fig. 13 illustrates the construction of a typical joint; the correction spring is above the joint and the spring element below. The individual spring elements were all completely calibrated, assembled, and set for initial load and closure point before they were put into the tower. All connections were joined thoroughly with medium hard solder. The pins shown were inserted for construction purposes only. The nuts and turn buckles were secured with solder after final adjustment to prevent their loosening during vibration of the model.

The adjustment of the tie-rods gave the only real difficulty in the entire work of building the model. This had to be done largely by gaging

FIG. 14.—1 :25 SCALE MODEL ON NEW SHAKING-TABLE

the uniformity of tension approximately by touching the rods laterally, and depending on the results of load-deflection tests for making the final adjustments. Even after several trials the tension in the model was still found to be somewhat uneven.

The completed model and shaking-table are shown in Fig. 14. The member representing the horizontal circular girder was made of $\frac{1}{4}$-in. hollow square tubing. It was placed inside the tank for convenience, being so small relative to the tank diameter that its effect upon the water motions is unimportant. The choice of load-deflection characteristics for the model was based upon the behavior of various designs in earlier tests, where simple harmonic motion was used to simulate the earthquake motion. It was found subsequently that the choice of a closure point was not ideal (see heading, "Earthquake Tests; Undamped Spring-Equipped Tower, Tested Parallel to Sides; Motion N. 40° E.").

Fig. 15 shows the load-deflection characteristic experimentally determined by pulling the model in the direction shown. (It is to be noted that loads, deflections, and periods throughout are given in terms of the full-sized structure, in order to avoid confusion as to scale relations.) A slight shifting of these curves relative to one another vertically would make them coincide very well, the maximum shifting required being about 1 200 lb. In the model, this amounts to about 35 grams, and represents the variations in adjusting the initial tension in the twenty-four tie-rods. Considering the small scale used, it is doubtful whether much better uniformity of rod tension can be obtained practically, and there is no reason to believe that an actual full-sized structure would be more uniformly adjusted as to tension by a field gang.

Shaking-table tests, first in the A-direction and then in the C-direction, showed no appreciable difference in dynamic characteristics. Indeed, this is what one would expect, since the natural period of the structure varies approximately with the square root of the area beneath the load-deflection curve, which means that for amplitudes greater than 2 or 3 in. the period is not appreciably affected by the variations in the four curves.

Damping was simulated by a solid friction device attached directly to the tank (Fig. 16). It was not considered possible to obtain reliable results by introducing friction into the twenty-four individual spring elements on such a reduced scale. The solid friction used in testing would closely approach the type of damping contemplated for actual construction. (See Fig. 17 for sectional assembly of a large unit equipped with damping device.) In fact, it is possible that a satisfactory element might be built more economically with pure solid (sliding) friction instead of the piston and cylinder arrangement shown in Fig. 17.

The choice of solid friction (or the equivalent) as a damping medium instead of the more conventional damping proportional to velocity (or some power of velocity) is justified by a consideration of the stresses that might be set up by the latter in the case of fast ground movements. Constant-force damping limits the stresses to those produced by deflection, plus a constant times the fixed damping force, regardless of the rate of change of deflection.

It will be noticed that the damping device shown in Fig. 16 does not exactly represent the damping to be derived from the spring elements, because the damping action of the latter is absent over the range of tower deflections indicated by Section A of Fig. 10. The difference in

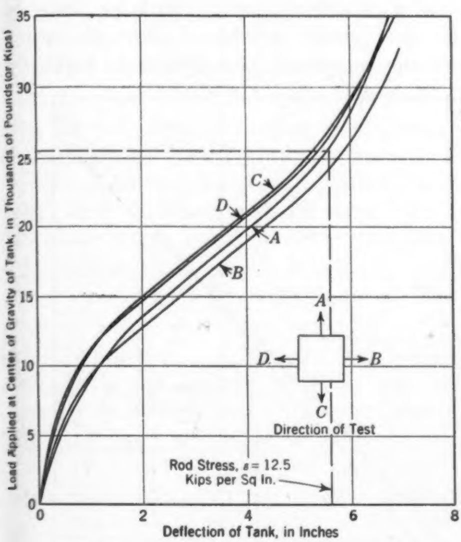

FIG. 15.—LOAD-DEFLECTION CURVES FOR TOWER EQUIPPED WITH SPRINGS

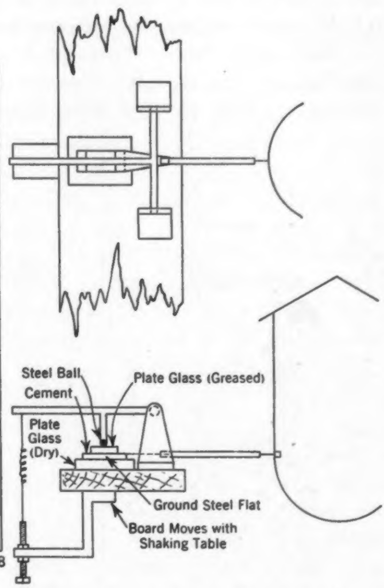

FIG. 16.—DIAGRAM OF FRICTION DEVICE

effect is small, however, and simply means that the damping force observed in the laboratory tests should be increased by 8%, or by 10% at the most, to secure the équivalent damping force required of the spring ele-

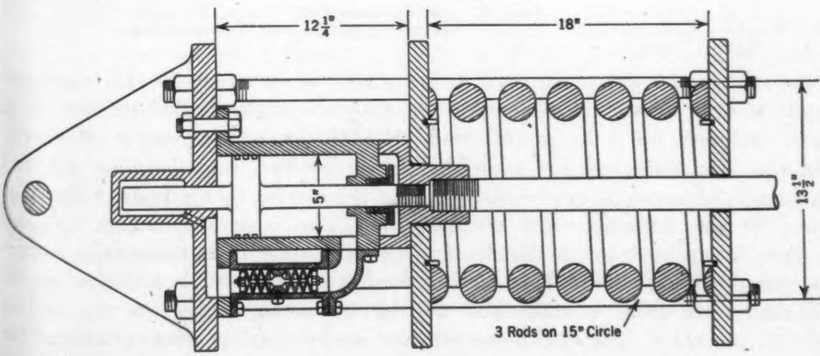

FIG. 17.—SHOCK-ABSORBING ELEMENT FOR TANK TOWER

ments for the same degree of earthquake resistance. (This has been generously allowed for in Conclusion (8), "General Conclusions Drawn from Research.")

The structural design assumed for this investigation is shown in Fig. 18. This design gives a calculated stiffness to the tower of 23 kips per in. of deflection before the springs begin to act.

The model was transformed into a standard tower for later tests by connecting the tie-rods directly to the joints instead of through spring elements. Fig. 19 (full line) shows the measured load-deflection curve for

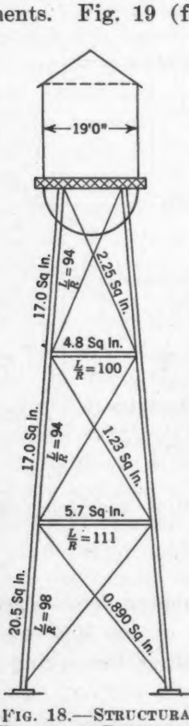

FIG. 18.—STRUCTURAL DESIGN OF PROTOTYPE; TESTS, PART II

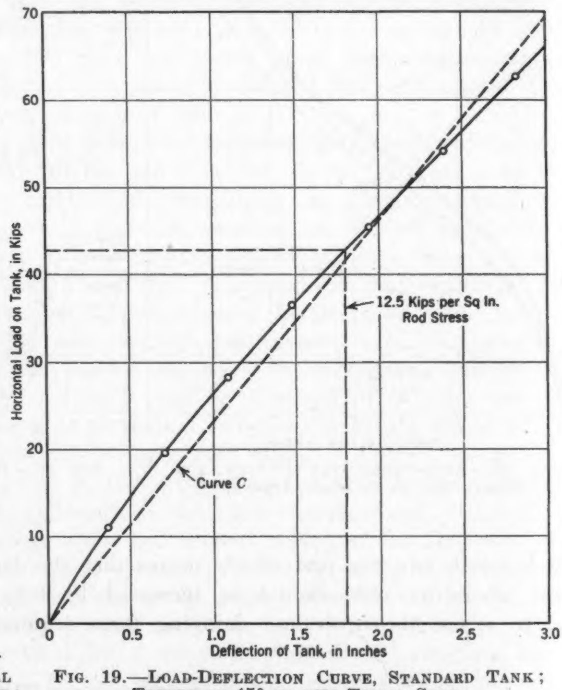

FIG. 19.—LOAD-DEFLECTION CURVE, STANDARD TANK; TESTED AT 45° TO THE TOWER SIDES

this condition. The calculated stiffness of the structure in this investigation is 44% greater than that of the standard tank studied in Part I; it was designed for a horizontal load of 43.5 kips at the center of gravity of the tank, whereas the tower originally studied was designed for 19.5 kips at the same unit stresses (18 kips per sq in., rod stress). The purpose of the stiffening was twofold: (1) The structure had to be built somewhat stronger to enable it to withstand large deflections as a spring-equipped tower; and (2) it was desired to learn just how much benefit would result from a reasonable degree of strengthening in a standard tower. Curve *C*, Fig. 19, represents the load-deflection curve computed for the design shown in Fig. 18. (The calculations were made assuming no initial tension in the tie-rods.)

Testing and Recording Procedure.—The model (see Fig. 14) was first tested statically to obtain the data in Fig. 15. Then it was mounted on the table of the shaking-machine so that the motion of the base was applied parallel to the tower sides. Later, the tower was turned through 45° so that the motion was applied diagonally for reasons given subsequently. All tests (including those for load deflection) except one, were made with the tank full of water.

The motions used for testing the model dynamically were taken from the data on the Long Beach earthquake of March 10, 1933. These valuable data consist of tabulations of ground displacements at intervals of 0.120 sec for the three components of that earthquake, as calculated from the accelerograms recorded at the Subway Terminal Building, in Los Angeles, Calif. The two horizontal components were plotted on cross-section paper and, after eliminating some very long period waves which were obviously unimportant, a polar plot was made for each component. The polar plots were made into optical cams[8] for driving the machine by cutting them along the plotted lines representing the ground motion. The cam for the component, N. 40° E, is seen in Fig. 14. No tests were made with the vertical component. (There is no direct evidence of dangerous earthquake stresses in the columns of steel tank towers due to vertical motions. Such column failures as have occurred appear to have resulted from poor details and not from true column failure in the proper sense of the word. However, it would be very desirable to extend the investigation to include earthquake effects in the vertical direction. From this standpoint a spring-equipped tower would behave substantially like a standard structure.) The two horizontal components are referred to throughout as the Los Angeles motions, N 40° E and S 50° E. The reader will understand that they are motions of the Long Beach shock as recorded in Los Angeles (37 miles from the epicenter).

A number of free vibration tests were made with the model as a spring-equipped structure (with and without damping), and as a standard structure. As in earlier tests, the procedure was a sudden release from a pulled-back position.

The procedure used in recording the motions during tests was essentially the same as that followed in earlier studies. For each test, the table motion, the deflection of the tank relative to its base, and time marks, were recorded simultaneously on one photographic record. Free vibration tests were recorded in the same manner.

The arrangement for testing and recording is shown in Fig. 20. A rigid wooden framework moving with the shaking-table carries the recording mirrors and lenses so that the tank deflections are recorded directly. The same frame also carried the friction device shown in Fig. 16.

Free Vibration Tests.—All free vibration tests were made diagonally to the tower, in order to be consistent with most of the earthquake tests. The maximum amplitudes of successive swings in the free vibration of the standard structure are given in Fig. 21(*a*) in terms of the percentage

[8] *Bulletin,* Seismological Soc. of America, July, 1936.

of the original pull-back before releasing, plotted against the pull-back amplitude. One observes here about the same degree of linearity as in the case of the smaller model investigated previously. The exact form of

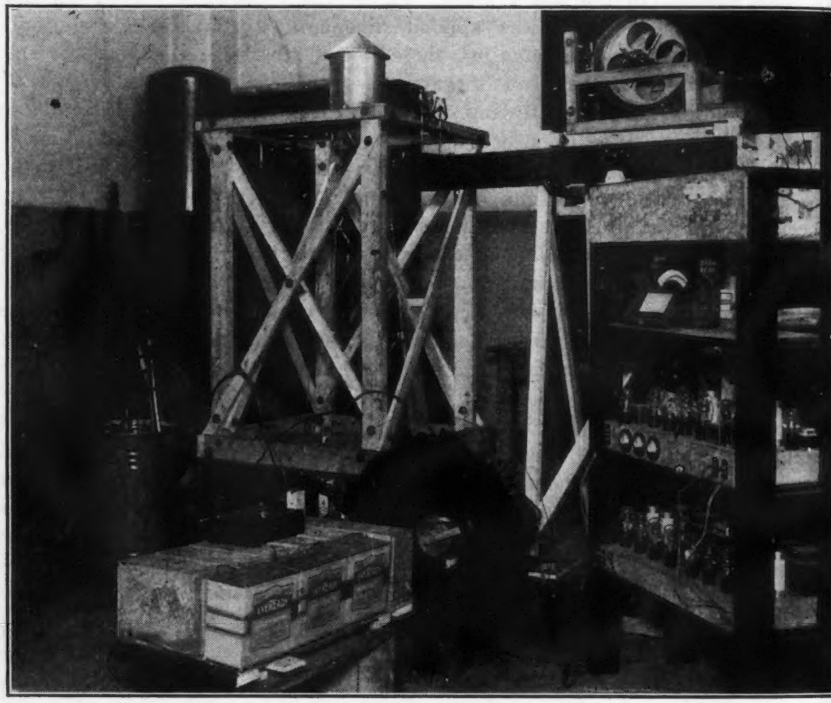

FIG. 20.—SET-UP FOR TESTING AND RECORDING ACTUAL EARTHQUAKE MOTION EXPERIMENTS

the free vibration is not the same as before because the present model represents a stiffer structure. The absence of damping action is shown by the persistence of large amplitudes in the 5, 6, and 7 maxima. (By "damping" is meant the dissipation of energy (usually in the form of heat) by the action of forces resisting motion.)

It must not be inferred that because the amplitude has dropped to 60% of the initial pull-back by the seventh swing that the stored energy has dropped to 60% of the original. The original energy has merely been changed from all potential to part potential and part kinetic (that is, the water is surging when the tank stands still at the end of a swing), the total energy being scarcely diminished in the first five to ten swings. Similar pull-back tests made by the United States Coast and Geodetic Survey[9] on full-sized structures show very little decrease in energy even after twenty to thirty swings of the tank. The absence of appreciable damping in standard tank structures and their foundations is the greatest

[9] *Special Publication No. 201,* U. S. Coast and Geodetic Survey, pp. 75-95.

hindrance to making them earthquake resistant by any ordinary means. From the free vibration tests, the principal period of vibration was found to be 1.3 sec (2-in. amplitude). The secondary period (mainly due to the water swinging in the tank) is about 2.7 sec.

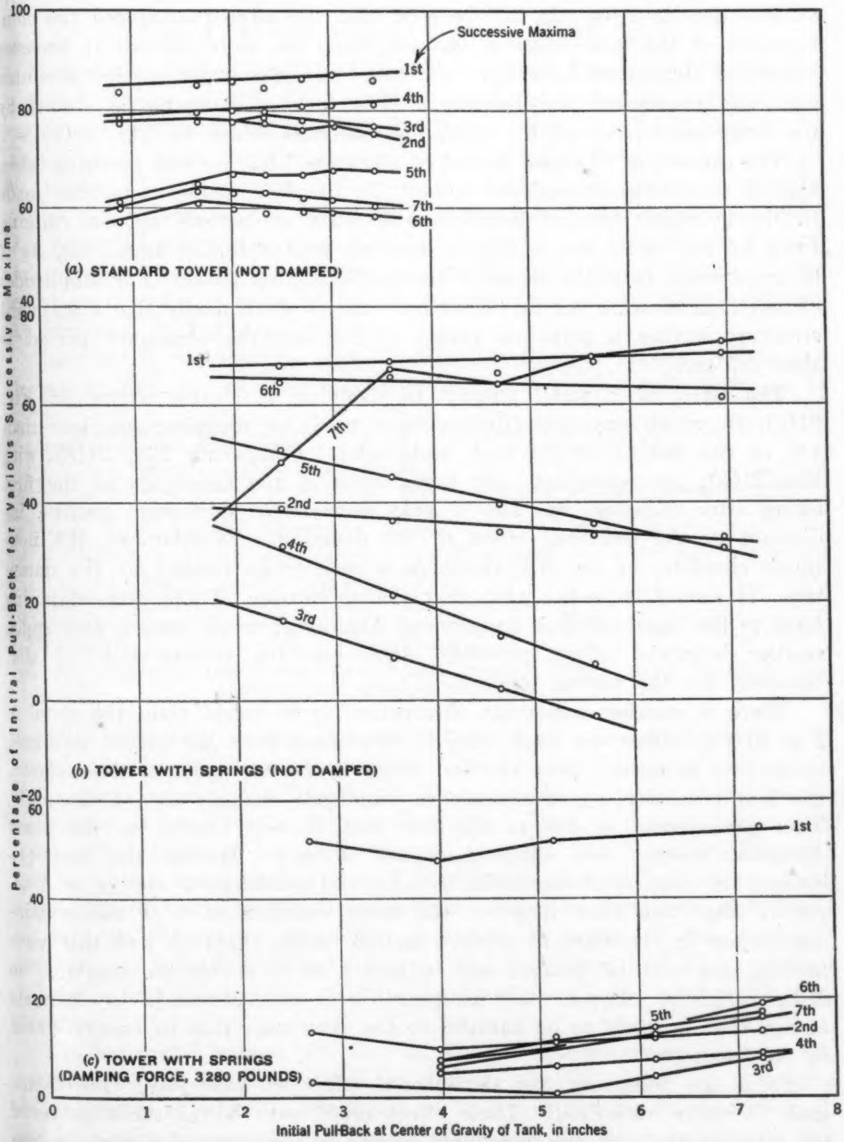

FIG. 21.—FREE VIBRATION OF TANK WHEN SUDDENLY RELEASED FROM A PULLED-BACK POSITION

Fig. 21(*b*) shows graphically the free vibration characteristics of the spring-equipped structure with no damping acting. Here, a pronounced non-linearity is observed, as evidenced by the great deviation of some of the plotted lines from the horizontal. Comparing Fig. 21(*b*) with Fig. 15, one can obtain a further conception of the relation between dynamical and statical non-linearity. It will be seen that the more pronounced the non-linearity of the load-deflection characteristic, the more difficult it becomes to predict dynamical behavior. (In each case, the water motion accounts for some dynamical non-linearity.) The lack of damping is shown by the large amplitudes of the sixth and seventh swings in Fig. 21(*b*).

The expression, "natural period of vibration," has no real meaning when applied to the spring-equipped tower. In the free vibration records made in the pull-back tests, a complete assortment of periods appear, ranging from 3.5 sec to 1.5 sec, which, in general, vary with the amplitude, as is to be expected from the shape of the load-deflection curve. For amplitudes of less than about 1 in., this structure behaves dynamically like a standard structure having a principal period of 1.5 sec; the secondary period is then 2.7 sec.

The effect of a small amount of damping is clearly shown in Fig. 21(*c*), in which case the friction force used for damping was less than 1% of the weight of the tank and water. Comparing Fig. 21(*c*) with Fig. 21(*b*), one notes not only a reduction in the amplitude of the first swing after releasing, but also a large decrease in subsequent swings, indicative of the beneficial effect of the damping. Furthermore, the non-linear character of the free vibration is seen to be masked by the damping. It should be noted, also, that the application of the same damping force to the standard tank structure of Fig. 21(*a*) would have a very much smaller beneficial effect, probably about one-fifth or one-sixth of that obtained for the spring tower.

There is another important observation to be made from the data in Fig. 21(*c*). After the large drop in amplitude from the initial pull-back to the first swing and from the first swing to the remainder of those shown, one might wonder why the decay in amplitude changes pace so abruptly. This phenomenon is due to the fact that friction applied to the tower dissipates energy only when the tower deflects. During the first two swings, the motion of the tower is large and considerable energy is dissipated; after this time, however, the water contains most of the remaining energy in the form of a wave motion inside the tank and this wave motion can only be damped out indirectly as it causes the structure to deflect slightly. Hence, the damping force acts primarily to dissipate energy that is likely to be harmful to the structure; that is, energy stored in the framework.

From the foregoing, the detrimental effect of baffle-plates inside the tank is easily visualized. Their effect could only be to throw more of the total energy into the structural frame and less into the wave motion of the water. In the case of very large and flimsy storage tanks, the

capacity of the tank shells to withstand the surging motion of the water may well be questioned. The shells of the standard elevated tanks of capacities not greater than 100 000 or 150 000 gal are most likely quite strong enough to stand the water forces, provided the strength of the roof is properly utilized to support the upper part of the shell.

Spring Element Design; Scale of Model, 1:25

Earthquake Tests; Undamped Spring-Equipped Tower, Tested Parallel to Sides; Motion N 40° E.—The behavior of the undamped spring tower was first investigated by applying the Los Angeles, N 40° E, earthquake motion to its base, the direction of motion being parallel to the sides of the tower. To insure the safety of the model, the amplitude of this motion was reduced by the factor, 0.72, of the actual earthquake amplitude. This factor is referred to throughout this paper as the "Amplitude Factor" and signifies the ratio of the testing amplitude to the amplitude of the actual earthquake as recorded by the accelerograph in the Subway Terminal Building, in Los Angeles.

Tests made by applying the described motion at various speeds are plotted in Fig. 22(*a*). The maximum deflection of the tank (relative to

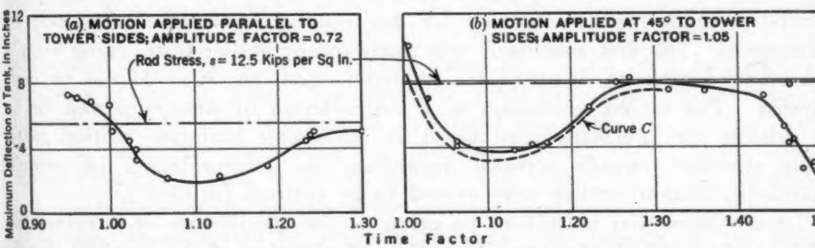

Fig. 22.—Maximum Deflection-Time Factor Curves, Tower with Springs; Not Damped; Los Angeles Motion, N 40° E.

its base) occurring during the application of the earthquake motion is plotted against the time factor. The "Time Factor" represents the ratio of the duration of the earthquake in the test to the duration of the actual earthquake as recorded by the accelerograph. Thus, a time factor of 1.10 denotes that T sec of actual earthquake occupied 1.10 T sec in the test; that is, a time factor greater than 1.00 indicates that the motion was more slowly applied than the actual earthquake. Since as previously mentioned, all data in this paper have been expressed in terms of the full-sized structure, no attention need be paid to scale relations, and all the curves may be interpreted directly as if the tests had been made on the full-sized structure.

The horizontal broken line in Fig. 22, and in subsequent diagrams, is a reference line for comparing the various sets of tests. It represents the deflection at which a rod stress of 12 500 lb per sq in. is developed, and by noting the position of the curve of tank deflection relative to this line one can interpret the results in terms of stress. Fig. 15 will also be

found helpful in this respect. The reference stress line was chosen at the break-over point of the load-deflection curve (see Fig. 15). To be consistent, the same stress was used as a reference line for the standard tower data (see Figs. 19 and 25). It is for comparison only and does not represent "safe" or "working" stress.

Fig. 22(*a*) indicates that without damping the spring elements cannot be considered very satisfactory because, although dangerous deflections are not reached anywhere on the curve, they certainly might be if the amplitude factors were raised from 0.72 to 1.00, or more. Two important conclusions were drawn from these tests. It was evident: (*a*) That damping should be present if the Los Angeles earthquake records can be accepted as representing a possible kind of motion to be found in Nature; and (*b*) that it would have been much better to have chosen a structure capable of about double the maximum safe deflection originally chosen. In other wards, instead of having the springs close at a deflection of about 6 in., they should close as far out as the structure can safely be deflected (15 in. is a practical figure from the standpoint of column bending and spring design, although 20 in. does not seem unreasonably large.)

These two conclusions emphasize clearly the desirability of applying actual irregular ground motions for determining earthquake resistance of structures. The first conclusion was suggested by earlier tests, using simple harmonic motion to represent the ground motion, but it could not be confirmed. The second conclusion is a contradiction of what appeared to be a suitable spring-closure point based on the simple harmonic motion tests. The cautions already stressed regarding the interpretation of simple harmonic motion studies were proved to be entirely justified.

Some discussion must here be given to the significance of varying the time and amplitude factors. Variation of the time factor can be interpreted in two ways: (1) With a given structure, varying the time factor makes it possible to find what effect the earthquake would have if it happened faster or slower, but with the character of the motion preserved as recorded; and (2) with a given earthquake, varying the time factor has the same effect as changing the natural vibration period of the structure. In other words, by varying the time factor the experimental results can be generalized considerably, whereas if only a time factor of unity is used the results can be applied only to one particular earthquake acting on one particular structure.

The significance of varying the amplitude factor is obvious. It makes it possible to generalize the results in another direction, so that the effect of the same motion changed in amplitude can be seen.

Fig. 23(*a*) represents a series of tests following those previously described, the purpose being to find how the behavior of the structure would vary with changing amplitudes of the same earthquake motion. The time factor was held constant. These results show that the maximum tank deflection is very sensitive to small changes of earthquake amplitudes when

no damping is present. They emphasize the correctness of Conclusions (*a*) and (*b*), cited in discussing Fig. 22(*a*). No attempt was made to carry the amplitude factor near to unity for fear of over-stressing the model.

Earthquake Tests; Undamped Spring-Equipped Tower, Tested Diagonally; Motion N 40° E.—Since there were not sufficient funds available to

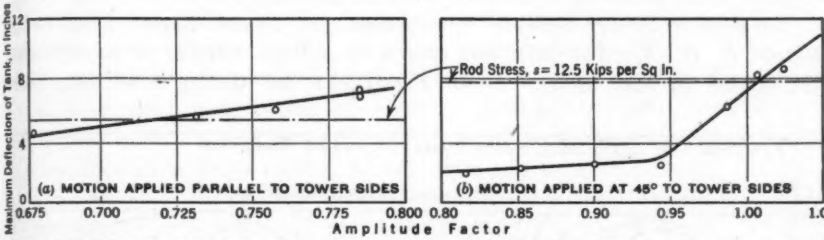

FIG. 23.—MAXIMUM DEFLECTION-AMPLITUDE FACTOR CURVES, TOWER WITH SPRINGS; NOT DAMPED; LOS ANGELES MOTION, N 40° E; TIME FACTOR, 1.02

permit building and testing a model to represent the design suggested by the tests just described, it was decided to turn the model so that the motion could be applied diagonally. In this position, the safe deflection is increased by $\sqrt{2}$, and it was thought that the results would be interpretable.

Fig. 22(*b*) (full line) is a plot of the results of a series of tests with earthquake motion at N 40° E applied diagonally to the tower and with an amplitude factor of 1.05. The reference stress line will aid in comparing with Fig. 22(*a*). In order to determine how well the diagonal tests could be used for interpreting the behavior parallel to the tower sides, Curve 1 was computed from the data of Fig. (22*a*), as if the structure were linear; that is, it represents the behavior of the tower under the same conditions, as computed by compounding the tests parallel to the sides. The agreement indicates that it is probably safe to accept the results of the diagonal tests as also applying to tests parallel to the sides of the same tower, by resolving into components as for a linear system.

Of course, a direct interpretation can be made by noting that the diagonal tests merely represent a tower with a load-deflection characteristic somewhat different from Fig. 15. It makes no difference in what direction this characteristic is taken to be; the behavior is dependent only on the moving mass and the character of the load-deflection curve.

From this study, it was concluded that applying the test motion diagonally would serve the purpose quite well. Conclusions (*a*) and (*b*), cited in connection with Fig. 22(*a*), were further substantiated.

A set of tests was then made to find the effect of changing the amplitude factor when shaking the structure diagonally. The results are plotted in Fig. 23(*b*). The form of the curve here closely resembles that obtained by varying the amplitude of the simple harmonic motion applied in earlier experiments (see Fig. 12(*a*)). Fig. 23(*a*) would show the same characteristics had the curve been extended far enough to the left. As long as the deflections of the structure do not pass the upper break in Fig. 15

(or the corresponding point for diagonal tests), the shape of the part of the load-deflection curve beyond this break has no effect upon the results, and one can just as well imagine that the break has been moved farther out to the right, as it should be. (The upper break in Fig. 15 is caused by the closing up of the spring elements. Its position can be varied between wide limits by a proper choice of springs.)

Earthquake Tests; Damped Spring-Equipped Tower, Tested Diagonally; Motion N 40° E.—The principal results of a large number of experiments are plotted in Fig. 24(*a*). As in Fig. 22(*b*), the testing conditions were

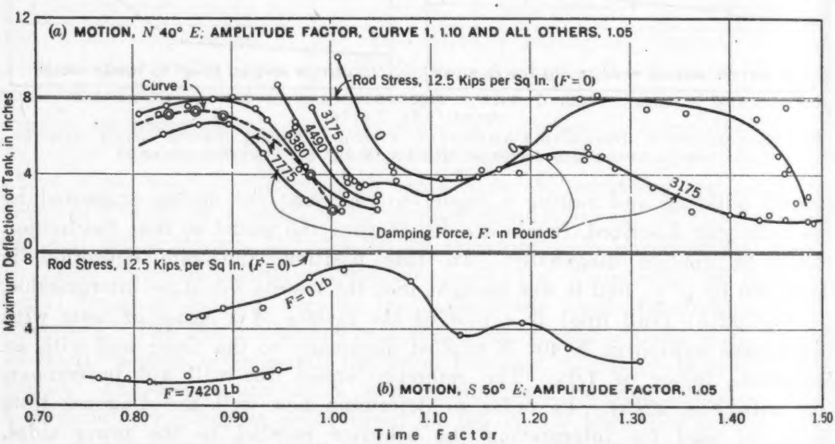

FIG. 24.—MAXIMUM DEFLECTION-TIME FACTOR CURVES, TOWER WITH SPRINGS; DAMPING FORCE F, AS SHOWN; LOS ANGELES MOTION APPLIED AT 45° TO TOWER SIDES

the Los Angeles motion, N 40° E, applied diagonally with an amplitude factor of 1.5 (except as noted in the case of Curve *C*). The variation in time factor was extended considerably below unity when the maximum amplitude was made small enough by damping to insure the model's safety.

Within the limits of the damping force applied in these tests the maximum tank deflection progressively decreased as the damping was increased, the beneficial effect tapering off as the damping became greater. Since the maximum stresses set up in the tower members result from the maximum tower deflection and the frictional force acting together, it is obvious that there must be some optimum frictional force beyond which the reduction in maximum amplitude due to a further increase in friction will be more than offset by the additional stresses produced by the increase in the friction itself. That this point was not reached in the tests plotted in Fig. 24, will be seen by noting that increasing the damping force F, from 6 380 lb to 7 775 lb (or 1 395 lb) reduced the maximum deflection by about 1.2 in., which by Fig. 15 amounts to a reduction of more than 3 500 lb in the pull on the tower due to deflection; that is the net gain in

reducing tower stress corresponds to a horizontal load of more than 2 000 lb at the tank.

The reasoning just given leads to the important consideration that most likely there are load-deflection characteristics much more desirable than that chosen for this particular study. Thus, it seems fair to conclude from the data obtained that the same structural design, equipped with somewhat more limber springs and adequate damping, would exhibit considerably lower stresses under the same earthquake conditions. The economic considerations involved in the choice of design could not be investigated with the funds available. Here is perhaps the most inviting field for future research, and it should be explored thoroughly if the new type of construction is to be applied to any extent in practice.

One set of tests was made with the amplitude factor increased to 1.10 to observe the sensitivity of the damped structure to amplitude changes. Curve 1 in Fig. 24(*a*) is a plot of the results of these tests (frictional force = 7 775 lb). The maximum of this curve is only about 10% greater than that of the corresponding curve for amplitude factor (= 1.05), indicating that the amplitude sensitivity has been reduced to a reasonable value by the damping action (compare Figs. 23(*a*) and 23(*b*)).

Limited as the data of Fig. 24(*a*) of necessity had to be, they demonstrate definitely the possibilities of the damped spring-element type of construction as a practical engineering approach to the problem of building earthquake-proof tank towers, and perhaps some other types of engineering structures as well.

Earthquake Tests; Spring-Equipped Tower, Tested Diagonally; Motion S 50° E.—The effect of the S 50° E component of the Los Angeles motion on the spring-equipped structure was found to be much less than that of the N. 40° E component. Fig. 24(*b*) is a plot of the results of tests made under the same conditions as those in Fig. 24(*a*) except for the different earthquake component. The tank motion proved to be so much more gentle than that for the other component that, after exploring the response of the undamped structure over a wide range of time factors, it was decided

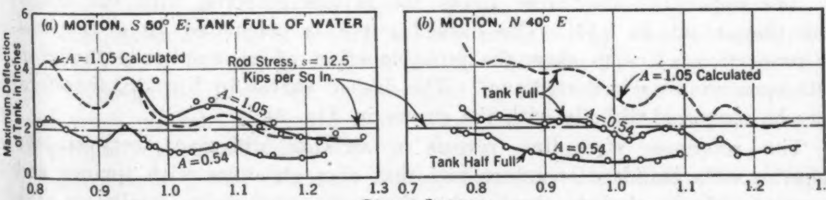

FIG. 25.—MAXIMUM DEFLECTION-TIME FACTOR CURVES, STANDARD TOWER; NOT DAMPED; LOS ANGELES MOTION APPLIED AT 45° TO TOWER SIDES; AMPLITUDE FACTOR, *A*, AS SHOWN

to limit the damping tests to one set (7 420 lb of frictional force). The significance of these curves is obvious. (Compare the relative effects of the two components on the standard structure, Figs. 25(*a*) and 25(*b*)).

STANDARD TOWER; SCALE MODEL, 1: 25

Earthquake Tests; Standard Tower, Tested Diagonally, Motion S 50° E.—It was felt that this research would be somewhat inconclusive without some experiments giving a comparison of the dynamical behavior of a standard rod-braced tower with that of a spring-equipped tower under identical earthquake conditions. Accordingly, the model was made to represent the standard structure described in connection with Figs. 18 and 19. The S 50° E component was first applied because it was expected to be the gentler of the two. The motion was applied diagonally to the tower to allow the maximum possible safe deflection. (In the case of the standard tower, the load deflection characteristic is practically identical in all directions, so that the tests described herein apply equally well to other directions.) The amplitude factor was fixed at 1.05 as before and a set of tests was made by varying the time factor from 1.28 to 0.96, at which point it was decided that it might be dangerous to the model to proceed farther without reducing the amplitude factor (see Fig. 25(*a*), upper full-line curve).

In order to explore the response for smaller time factors, the amplitude factor was reduced to 0.54 and tests were made over a wide range of time factors. The results are plotted in the lower full-line curve in Fig. 25(*a*). Knowing the approximate linearity of the structure from statical and free vibration tests, it was assumed that the results obtained with the reduced amplitude factor could be extended fairly well to larger amplitudes by computation. The dashed curve in Fig. 25(*a*) shows how this assumption agrees with the facts. This curve was obtained by simple proportion from the lower full-line curve, each ordinate of the former being $\frac{1.05}{0.54}$ times the corresponding ordinate of the latter.

Earthquake Tests; Standard Tower, Tested Diagonally; Motion N 40° E.—It seemed wise to apply the N 40° E component with the reduced amplitude factor of 0.54 also, because of the danger of overstressing the model. Fig. 25(*b*) is a plot of the results obtained with this motion.

The upper full-line curve shows the results observed with the amplitude factor set at 0.54. The dashed curve is calculated from the first by proportion so as to show the probable effect of an amplitude factor of 1.05 upon the standard structure. The dashed curves in Fig. 25, therefore, may be compared directly with the curves in Fig. 24.

The reference stress-line reveals a striking difference between the behavior of a standard structure and that of a structure with springs and damping. It is obvious that under the same earthquake conditions, the stresses in the standard tower reach values two or three times as great as those in the spring-equipped structure having about 7 000 lb of friction damping. This situation is brought about as follows: The rigidity of a standard tower, by giving it a relatively short natural period of vibration, results in considerably smaller earthquake deflections than are produced in a moderately damped spring-equipped tower with its longer period.

On the other hand, the rigidity of a standard structure is so much greater than that of a spring-equipped tower, that the beneficial effects of the decreased deflections are more than offset by the large forces required to produce them.

In Fig. 25, one is impressed by the small decrease in maximum tank amplitude as the time factor changes from 1.00 to 1.15. Recalling the second interpretation of the time factor variation stated in discussing Fig. 22, it is clear that, as the time factor goes from 1.00 toward the right, the results indicate the probable behavior of a structure stiffened even further than the one chosen for the present research. Thus, at Time Factor 1.15 the results may be interpreted as closely representing the effect of the earthquake upon a tank structure having a natural period $\frac{1}{1.15}$ times that of the structure described in Figs. 18 and 19, and since the natural period varies inversely with the square root of the tower stiffness, the new stiffness would be $(1.15)^2 \times 23\,100 = 30\,500$ lb per in. This is 1.9 times as stiff as the standard structure studied in earlier investigations. To achieve this stiffness requires a surprising area of steel.

Thus, it must be concluded that the degree of direct strengthening that is generally applied cannot be expected to insure the ability of the structure to withstand the earthquake motions in questions. (In this connection it is interesting to note that if the areas of the rods in the lower two panels, Fig. 18, are doubled, leaving the top rod areas as shown, the resulting earthquake unit stresses will be increased by more than 15 per cent. The rod areas in Fig. 18 have been chosen to give a safe deflection much greater than that achieved in the usual "quake-proof" structure, in which no particular attention is given to balanced design. Furthermore, the unit stresses used in this design were very conservative—20 kips per sq in. rod stress at 10% gravity loading. If higher unit stresses are used, the natural period will be lengthened.) Although only the motions of one particular earthquake were available for these studies, the fact that the same conclusion was reached from the results of the application of simple harmonic motion in earlier experiments indicates that it cannot be ignored in any serious discussion of quake-resistant design for elevated water-tanks.

That strengthening of towers, as now practiced by some designers, does generally serve to produce a better design with sounder structural details is not to be gainsaid; indeed, improvement in that direction is commendable and much needed. It must not be imagined, however, that the process has a rational dynamical basis.

Earthquake Tests; Standard Tower with Tank Half Full, Tested Diagonally; Motion N 40° E.—The lower full-line curve in Fig. 25(*b*) represents a series of tests made with the tank half filled with water. All other conditions were kept the same as for the tests plotted in the upper full-line curve. These results help to answer the much-asked question about the behavior of a partly filled tank.

The reduction in deflection in the half-full tests must not be attributed directly to the change in mass. It is brought about solely by the shortened

period resulting from the decrease in the ratio of mass to tower stiffness. Had the stiffness of the structure at the same time been reduced by one-half, the behavior would be closely represented by the upper full-line curve in Fig. 25(*b*). The dynamical behavior is purely a matter of the natural periods of the structure and is not directly dependent upon its physical proportions.

That this is so may be verified by replotting the curve for the half-full tank, giving each point the same ordinate but multiplying its time factor by $\sqrt{2}$. This procedure is equivalent to treating the results as applying to a full tank with a natural period $\frac{1}{\sqrt{2}}$ times that of the actual full tank studied. The result will be a curve to the right of Time Factor 1.08 coinciding very well with the experimental curve for the full tank.

Extending this reasoning in the direction of added mass instead of decreased mass, it is evident that unless the tower stiffness increases in proportion to the capacity of the tank, the natural period will be greater than that of the structure taken as the prototype in the present research, which is the same as working to the left of Time Factor 1.00 in Fig. 25. Keeping in mind the fact that the natural period of the structure tested in these studies was about 1.3 sec, with a stiffness 1.44 times that of the original tank, one cannot avoid the conclusion that, for larger tanks and for tanks on higher towers, the natural periods cannot economically be made short enough to help matters very materially. (Mr. D. S. Carder has presented valuable data[10] on observed tank periods. In this connection, it is important to bear in mind that a small amount of initial tension in the rod-bracing can produce a natural period for small amplitudes which is considerably shorter than the natural period for amplitudes such as exist during an earthquake.) Besides, one must consider the fact that non-rigid foundations will invariably lengthen the period of a structure, which might annul, partly or totally, the beneficial effect of the increased tower stiffness.

It should not be inferred that the lengthened natural period brought about by the introduction of spring elements is a desideratum; in itself it is to be regarded rather as an unavoidable, and not necessarily desirable, effect resulting from the necessity of providing a greater range of deflection to the structure. The increased limberness makes it possible to introduce damping action into the system and requires a far smaller damping force than would be needed for a stiff structure.

Comparison of Earthquake Stresses in Standard and Spring-Equipped Towers

In comparing the earthquake stresses in the standard and spring-equipped structures one must take account of the fact that, in Nature, the two components of ground motion occur simultaneously, whereas they were

[10] *Bulletin*, Seismological Soc. of America, January 1936, Tables I and II; also, *Special Publication No. 201*, U. S. Coast and Geodetic Survey, pp. 75-95.

treated separately in the laboratory work. This means, of course, that the orientation of the structure in question relative to the recorded components of motion is of importance. Since it is beyond the scope of this paper to compare the two structures under all possible orientations, the writer has chosen to compare them only when oriented in such a manner that the standard structure has every advantage over the spring-equipped structure.

A study of Figs. 24 and 25 and of the laboratory records shows that, if the recorded ground-motion components are simultaneously applied parallel to the tower sides for both structures, the standard tower will be stressed about 25% less than at its worst possible orientation, whereas the spring-equipped tower is oriented so as to receive within 5% of its worst possible stresses. The comparison will be made on the basis of an amplitude factor of 1.05.

The stress calculations involve only straightforward statical analysis, once the deflections are known from the experimental data (the gravity effect must not be ignored in computing the stresses). The deflections of the standard tower will be substantially the same regardless of the direction of the applied earthquake motion, because the tower stiffness is substantially the same in all directions. The maximum deflections of the damped spring-equipped tower, however, will be somewhat greater parallel to the sides than in the diagonal direction for the same earthquake motion. In the following data, the deflections parallel to the sides are assumed to be 35% greater than the diagonal deflections for the spring tower, which the writer considers a generous allowance. Data are also given for the case in which no such assumption need be made. In the absence of complete data to determine the value of 35%, the writer has set what seems to be an upper limit, based on his experience with the system. He prefers the more direct interpretation of stresses given in the last paragraph of this section, since the element of estimation is there eliminated entirely.

For the standard tower under the conditions specified, the rod stress at a time factor of 1 would be more than 28 kips per sq in.; and for the worst case ($P = 0.82$) the rod stress would be about 40 kips per sq in., which is beyond the normal yield point. It is not necessary to postulate any great increase in the violence of the ground motions closer to the epicenter in order to explain the failures which occurred, knowing that few of the existing structures could safely withstand as much deflection as the structures studied herein, and that most of the larger ones corresponded to time factors of considerably less than unity.

As a matter of fact, some rods were stretched permanently in a tank tower situated 3.7 miles from the Subway Terminal Building, in Los Angeles, where the motions used in this research were recorded. This structure is a 100 000-gal tank on a 100-ft tower, and, at the time of the Long Beach earthquake, was braced for wind velocity of 100 miles per hr. At that time, it corresponded to a time factor of about 0.9. The calculated maximum rod stress for this structure, based on the experimental data in Fig. 25, proves to be 56 kips per sq in. for an amplitude factor of

1.05 and a time factor of 1.00, or 53 kips per sq in. for the earthquake as it was recorded. (These high values, of course, are fictitious, since the elastic limit of the material would be passed at lower stresses.) Failure might easily occur, therefore. Several other tank structures within a few miles of the Subway Terminal Building also showed evidence of very high stresses. Since, in these cases, there were indications of flaws in material, workmanship, or details, they are not cited herein as numerical examples.

The stresses set up in the damped spring-equipped structure, under identical earthquake conditions (allowing 35% extra deflection), prove to be quite low in comparison. With a damping force of about $\frac{7\,500 \text{ lb}}{\sqrt{2}}$ (parallel to sides) the rod stress at a time factor of 1 would be less than 10 kips per sq in. (compared with more than 28 kips per sq in. in the standard tower). In the worst case ($P = 0.88$) the rod stress would be less than 20 kips per sq in. (compared with a stress beyond the yield point in the standard tower). Thus, even with a generous margin in favor of the standard structure, the damped spring-equipped tower shows a minimum safety factor of at least 2 over the standard tower. A 25% increase in damping would raise this safety factor to above 2.25; or, if a more direct interpretation is desired (in which no assumption need be made as to deflection increase parallel to sides), the following will suffice: Given a tower whose load-deflection characteristic parallel to the sides is the same as that of the tested structure in a diagonal direction, and given a damping force of 7 500 lb, the rod stress in the worst case ($P = 0.88$) would be less than 19 kips per sq in. (minimum safety factor, greater than 2). It is to be remembered that the assumed orientation is such as to give the advantage to the standard structure by perhaps 20%, which was ignored in calculating the safety factors.

General Conclusions Drawn from Research

It is not intended to list herein a set of conclusions that may be read casually and without a careful study of the supporting text. Such a procedure would be worse than useless; it might even be dangerous. Certain general conclusions, however, can be drawn from the results of this research, provided the limitations and cautions already set forth have been thoroughly digested. These conclusions are:

(1) The standard type of elevated tank tower now in common use is poorly adapted for withstanding earthquakes of destructive intensity, both on acount of the absence of damping action and of its inability to withstand large deflections.

(2) A moderate degree of strengthening does not, and can not, improve the earthquake resistance of a standard tower very materially; that is, if the recorded motions of the Long Beach earthquake of 1933 can be taken as indicative of what may be expected in future earthquakes. The data from these motions indicate that a properly balanced strengthening is somewhat

better than a straight wind design, but in the absence of a more general knowledge of possible earthquake motions, this conclusion cannot, of course, be accepted without reservations. (By "balanced strengthening" is meant strengthening of the tower bracing members in proportion to the loads they carry, as distinguished from the common practice of using the same rod size in two or more panels. The safe deflection of a standard 100-ft tower can sometimes be increased as much as 50% by a balanced design of the bracing.)

(3) It would be impractical to provide sufficient safe deflection to insure the safety of the structure under all conditions by using springs without damping. Given a safe deflection of about 15 in., the undamped spring tower would probably fare better, on the average, than the standard structure but its safety factor would not be dependable.

(4) The presence of sufficient damping in a properly designed spring-equipped tower gives it a safety factor in stress of at least 2 over the standard structure which has been designed for a loading equal to $0.1g$. Damping also provides a safety factor in the duration of violent motion that the structure can withstand—a quantity difficult to express numerically but of great importance. The damping required in the particular design investigated was found to be small enough to be easily achieved in practical design (see Fig. 17).

(5) The results obtained are restricted in scope, and they cannot be extended safely to apply to structures of much greater or much smaller proportions than the tower actually studied. One must not resort to "engineering judgment" in dynamical problems unless one has had considerable experience with them. Practical limits for applying the present results might be 50 000 to 100 000-gal capacities and 150 to 100-ft tower heights, respectively.

(6) The introduction of the proper spring elements does not endanger the stability of the tower structure. Considered as a three-dimensional framework, it is necessary to investigate the effect of possible dissymmetry due to accidental differences in action between the elements on opposite sides of the tower. Although the model did not exhibit serious torsional effects from lack of symmetry, a good engineering design should specify horizontal panels of light rod-bracing at the strut levels, in order to eliminate any undesirable twisting effects which might be produced if some of the damping devices failed to act properly. (The conventional spider can be adapted for this purpose.)

(7) Baffle-plates inside an elevated tank would be detrimental within the limits of the tank size specified in Conclusion (5).

(8) For designing structures within the foregoing prescribed limits of size, the following rules should result in a safe design:

(a) Design the structural frame for a static horizontal load of one-tenth the weight of the tank and water applied at the center of gravity of the tank. The columns and struts should be designed so that the rod-bracing would fail first if the structure were tested to destruction by static horizontal loading.

(*b*) Provide a set of springs such that, with the initial compression set for a horizontal force at the tank equal to one-fiftieth of the weight of the tank and water, the tower can deflect at least 15 in. parallel to its sides before the springs close.

(*c*) Design the stiffness of the springs such that at the point of closure the horizontal load is approximately equal to the design load used in Conclusion (8*a*).

(*d*) Design the individual spring elements so that the total tower deflection is about equally distributed between the panels (to avoid high bending stresses).

(*e*) See that the bending stresses in the tower members are not excessive. Use pin-connected struts if necessary, and reduce eccentricity to an absolute minimum. The riser pipe should be equipped with flexible joints if necessary and must be able to follow the tower motions.

(*f*) The damping forces should be such that the energy dissipated by damping during a motion of the tank (parallel to the tower sides) from its equilibrium position out to the maximum safe deflection and back again, is approximately equal to 50% of the energy stored elastically when the structure is at the maximum safe deflection (parallel to the tower sides).

(*g*) The total damping force should be distributed among the spring elements in proportion to the stresses produced in the various rods by a static horizontal load at the center of gravity of the tank. The damping force need not be the same for an outward deflection as for the return to equilibrium, but that for the return stroke should not exceed about 85% of the wind-load initial compression setting; otherwise, all the rods may sag at times.

(*h*) The damping force should be fairly independent of velocity. It would be a good rule to make the damping force corresponding to a tank velocity of 30 in. per sec (relative to the base) not more than 25% greater than that calculated by Conclusion (8*f*). The damping may be produced by simple sliding friction if desired, in which case the kinetic friction should be made equal to that computed by Conclusion (8*f*).

Remarks

Relation of Tank Deflection to Ground Motion.—The graphs in Fig. 26 are reduced tracings of records made by applying the Los Angeles motion, N 40° E, to the undamped spring tower, the damped spring tower, and the standard tower, respectively. The time factor is nearly identical for all cases and the scales given are self-explanatory. A careful study of the nature of the tank motion relative to the ground motion in the three diagrams will be found helpful to those who are not well versed in dynamics.

In the first place, one observes that there is no simple relation between tank response and earthquake motion. The notion that statical force equal to the mass of the tank and water times the maximum ground acceleration can be used safely for design against earthquake is seen to be entirely without basis.

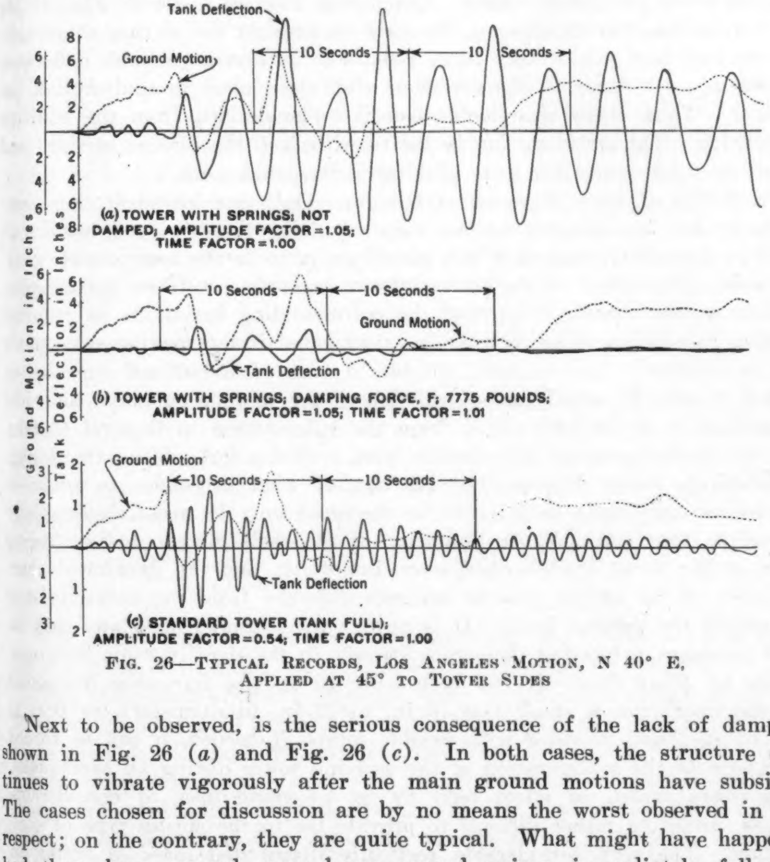

FIG. 26—TYPICAL RECORDS, LOS ANGELES MOTION, N 40° E, APPLIED AT 45° TO TOWER SIDES

Next to be observed, is the serious consequence of the lack of damping shown in Fig. 26 (*a*) and Fig. 26 (*c*). In both cases, the structure continues to vibrate vigorously after the main ground motions have subsided. The cases chosen for discussion are by no means the worst observed in this respect; on the contrary, they are quite typical. What might have happened had there been several more large ground swings immediately following those in the early part of the Long Beach earthquake can only be conjectured, but if such swings happened to be nearly "in step" with the already large tank vibrations shown in Fig. 26 (*a*) and Fig. 26 (*c*), collapse would have been almost certain.

It is to be regretted that the accelerograms taken in Long Beach proper could not be completely analyzed because of the overlapping of the components on the record. Consequently, there is no rational method by which to estimate the relative intensities at Long Beach and Los Angeles except by comparison of destructive effects, and this is admittedly not very satisfactory. However, it is fairly certain that the motions used in the

present research cannot be considered as representing the worse conditions to be designed against in an active earthquake zone, such as the West Coast. Hence, the desirability of adopting a generous and real safety factor is apparent.

In Fig. 26 (*b*) the beneficial effects caused by the addition of a moderate damping force are clearly shown. Comparing Fig. 26 (*a*) with Fig. 26 (*b*) and noting that the damping is the only variable in the testing conditions, one observes not only a very large reduction in maximum tank deflection, but also a quick decay in the vibration after the violent ground motion has subsided. Thus, there is a double benefit to be derived from the addition of damping. The structure is enabled to withstand the violent motions and is quickly made ready for more shaking if it should come.

Possibility of More Economical Designs.—Only one load-deflection characteristic was investigated in the tests of the spring-equipped tower. It would be remarkable indeed if this should prove to be the best possible characteristic. The effect of design variations in spring stiffness, initial compression, spring closure point, and the corresponding variations in required damping force need to be thoroughly investigated if the maximum economy is to be attained. For example, consider a tank of 150 000-gal capacity on a 125-ft tower; it would undoubtedly prove far more economical and just as satisfactory to deviate widely from the rules stated in General Conclusion (8), if the necessary information were available for guiding the design.

Extremely Large Tanks.—The question of what procedure to adopt in the case of very large tanks is to be answered only by research especially directed to that end. As the diameter of the shell increases, the surging action of the water must become more important, and the dynamical characteristics of the system may be entirely different from the characteristics observed in the present study. It is quite possible that baffle-plates may be found necessary to prevent dangerous stresses in the shell in some instances.

Size of Riser Pipe.—It has been assumed in the foregoing discussion that the riser pipe is small (say, 6 in, to 12 in. in diameter), so that if made of steel and provided with flexible joints, if needed, it can be forced to conform to the configuration of the swaying tower during an earthquake. Large risers, which in effect tend to act as stand-pipes of considerable stiffness, would be rather difficult to provide for in the spring type of construction. The very considerable vertically distributed mass of water in large risers presents a problem to which the results thus far obtained do not apply.

The Element of Chance.—The extent to which pure chance enters into the destructive effect of earthquakes is perhaps not generally appreciated: The orientation of the structure relative to the direction of the most violent motion; the location of the structure in the shaken region; the relation of the natural vibration period to the frequency spectrum of the earthquake; hidden structural defects; characteristics of tower parts at stresses beyond the elastic limit—all these are factors vitally affecting the behavior of the structure and all are matters of chance over which the engineer has no control whatever.

A careful consideration of the foregoing chance factors leads to an understanding of the difficulties confronting those investigators who have undertaken to formulate design rules based upon field studies of the effects of destructive earthquakes. For instance, there is the fact that a tank tower designed for a static force of $0.1g$ (the only one so designed to the writer's knowledge) survived the Long Beach quake safely. This fact, apparently, has led a number of engineers to accept, tentatively, the $0.1g$ rule as satisfactory for elevated tanks.

Now, had there been, say, forty or fifty tanks of random sizes designed by this rule and a like number designed for wind only, and had they all been fairly well scattered over the destructive region, then there would be a real basis for drawing conclusions as to the superiority of one type to the other from field studies. As matters stand, however, about the most that can be concluded definitely from examination of the effects of the Long Beach quake is that certain tanks did fail totally or partly; and, by examination of the damaged structures, weak points in the details can be brought to light and corrected in the future. To credit the methods of design for saving those structures which did not fail, would lead to some awkward contradictions.

Tanks for Municipal Supply.—It is unfortunate that few municipal storage tanks are small enough to come within the scope of the research described in this paper. As far as importance to the community as a whole is concerned, these structures should receive by far the most serious consideration. It is to be hoped that it will be possible some day to extend present knowledge to this vitally important field.

Acknowledgments

The writer wishes to acknowledge gratefully the helpful co-operation of all who have contributed to the success of this research project.

The Associated Factory Mutual Fire Insurance Companies and the Freeman Engineering Corporation initiated the research and helped to make possible all the investigations described in Part I, also contributing a very considerable sum toward the construction of the new shaking-table. The engineers connected with these organizations recognized the serious lack of dependable design data and questioned the propriety of applying rule-of-thumb design methods unless they could be shown to produce safe structures. Their helpful and sympathetic attitude has been a source of inspiration as well as a means of keeping the work on a firm practical basis.

Thanks are due to the Chicago Bridge and Iron Works and to the Pittsburgh Des Moines Steel Company, which provided a large part of the funds for the experimental work described in Part II. These funds were given for the purpose of increasing current knowledge of earthquake effects upon tank structures, with no thought of financial gain except to earthquake-ridden communities.

The U. S. Coast and Geodetic Survey has given invaluable aid in providing data on ground motions and on the behavior of actual structures

studied in California, including the data on the Long Beach earthquake of March 10, 1933. Its interest in the work is most gratifying.

The studies, which extended over a period of two years, were made by the writer in the Laboratory of Engineering Seismology of the Massachusetts Institute of Technology. By providing funds for the design and construction of the new shaking-table and for the services of the writer for the duration of the entire research, the Institute has carried by far the greatest part of the financial burden. The Division of Industrial Cooperation has administered the project without making the customary charges for overhead expense to the Institute. In addition, the writer has profited by the valuable advice and assistance of Dr. Vannevar Bush, C. B. Breed, M. Am. Soc. C. E., Professor A. V. deForest, and many other members of the Institute staff.

Especial thanks are due to the four engineers without whose loyal assistance and hard work this research project could not have been carried out with the same degree of economy and success—Messrs. Chester H. Hosmer, Albert L. Grass, George Parmakian, and George W. Hines, all graduates of the Institute.

The data in Table 1 were computed by Mr. H. A. Sweet, Engineer with the Associated Factory Mutual Fire Insurance Companies, of Boston, Mass. Figs. 8 and 9 are reproduced from the drawings used in securing the patents, which are to be administered on a non-profit basis by that organization. These patents cover the general concept of applying spring elements to engineering structures for the purpose of increasing their resistance against earthquakes. The purpose of these patents is to make it possible to prevent the improper use of the new construction; otherwise, no restrictions will be imposed.

AMERICAN SOCIETY OF CIVIL ENGINEERS
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PAPERS

HYDRAULIC TESTS ON THE SPILLWAY OF THE MADDEN DAM

By Richard R. Randolph, Jr.,[1] Esq.

Synopsis

Several interesting and unique hydraulic tests have been made in connection with the Madden Dam project located on the Chagres River, in the Isthmus of Panama. A number of features of the design of the spillway section, but principally a plan for dissipating the energy of the overflow at the toe of the dam, were developed from model studies. These studies are described in Part I.[2] In order to check the resulting predictions, apparatus as nearly similar as possible to that used on the models, was installed in the prototype spillway at apportionate positions, as described in Part II.

Field tests other than those on the spillway have also been made.[3] The discharge and loss in head through the needle-valve outlet conduits and the sluice-ways were measured. The efficacy of the air vents for these conduits was determined by measuring the quantity of air flowing through them at various heads and gate-openings.

PART I.—MODEL TESTS

The Project

Madden Dam is a part of the Panama Canal project, being built primarily to create a reservoir for storing water to use for lock operation and for controlling floods. Power development is small and more or less incidental. The dam is on the same river as that which forms Gatun Lake and is situated about 10 miles up stream from the point where the river flows into the canal.

Note.—Discussion on this paper will be closed in **September, 1937,** *Proceedings.*

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[2] Condensation of a report by the writer to the Governor of The Panama Canal on Madden Dam Spillway Model Tests, dated February 1, 1932.

[3] *The Military Engineer,* Vol. XXVIII, No. 162, November-December, 1936, p. 438.

The principal features of the Madden Dam project consists of the main concrete dam across the Chagres River, the power plant, an earth and gravel-fill dam continuing from the left abutment of the main dam; and several earth and gravel-fill dams across saddles around the rim of the reservoir. The main dam is of the massive concrete straight-gravity type, consisting of an overflow spillway section across the river, with abutment sections on either side (see Figs. 12 and 13). It is about 974 ft long at the top and about 220 ft high at the maximum section from the lowest point of the foundation to the top of the roadway. The spillway is divided into four 100-ft openings by three 12-ft concrete piers. Structural steel drum-gates, 18 ft high, are installed on the concrete crests in these openings.

The flood water passing over the spillway crest has a drop of about 173 ft to the toe at the maximum expected discharge of 260 000 cu ft per sec. The energy of this overflow will be more than 5 000 000 hp, and the velocity at the toe will be about 100 ft per sec. With this energy exerted at the base of the dam it was considered advisable to devise means of dissipation so that the velocity would be checked enough to eliminate erosion of the foundation rock.

The Model

During the summer and fall of 1931, a model of Madden Dam was built and tested at the Hydraulic Laboratory of the Colorado Agricultural College, at Fort Collins, Colo.[4] The model of the spillway was built on a scale of 1:72, or 1 in. (linear dimension) on the model equal to 6 ft on the prototype. The spillway section, with drum-gates installed in the crest, was reproduced accurately to scale. The power house was represented with the needle-valves discharging through the down-stream wall. The contours of the river bed below the dam were reproduced from surveys to represent the original rock and overlying earth and gravel deposits. (Described in terms of equivalent prototype dimensions, this model had four 100-ft gate-openings, a spillway crest at Elevation 232.0, and an apron width of 440 ft between side walls. The radius of the bucket is 60 ft. For a flow of 260 000 cu ft per sec, and the tail-water depth is approximately 80 ft.)

Model Tests

Dentated Sill Apron.—In the original proposed design of the spillway a concrete apron, extending about 120 ft down stream from the toe of the dam with a dentated sill developed by Professor Theodor Rehbock[5] at the end, was proposed. This apron was level and at a low elevation in order to follow the average rock level.

Tests on a model of this set-up showed that the sheet of water flowing down the spillway cut through the tail-water, which was of considerable depth, at all stages of flow, with very little surface disturbance or loss in velocity, and struck the dentated sill with terrific force. Fig. 1 shows a

[4] *Engineering News-Record,* July 14, 1932, p. 42.

[5] *Transactions,* Am. Soc. C. E., Vol. 93 (1929), p. 527.

cross-section along this apron (through the dentate) with the maximum expected discharge. Velocity measurements were taken at various points with a Pitot tube, and lines of equal velocity, converted to prototype scale,

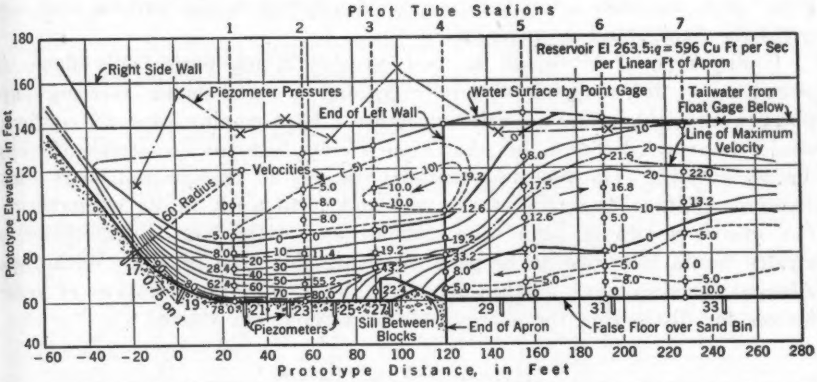

FIG. 1.—VELOCITY CONTOURS ALONG A SECTION THROUGH THE DENTALS; ORIGINAL MODEL DESIGN

have been drawn representing contours. The high-velocity stream is depressed by the back roll of water above the sill and elevated by the roller below the sill. This latter action, of course, relieves the river bed of erosion velocities. The upward deflected jet continues along the surface, however, at comparatively high velocities and erodes the river banks.

The next step in the experiments was to substitute a sill with a solid vertical face for the dentated form. Various heights of this solid sill were tried, and it was found that a height of 6 ft was just as effective in preventing erosion below the apron as the dentated sill 10 ft high. However, the surface flow conditions were unstable, the boil above the solid sill being much more irregular and fluctuating. The flow allowed through the dentates seems to stabilize this boiling, and there is little or no pulsation or surging in the tail-water, making the performance of the dentated sill more satisfactory.

It would seem that a sill under these conditions should be made only high enough to cover the range of the high velocities of the overflow jet along the bottom, and, consequently, its height would vary inversely with that of the dam, because the higher the dam the thinner the overflow sheet will be at the toe. For Madden Dam a satisfactory sill was one-thirtieth of the total height of the dam. The height of the sill varies directly with the depth of water discharging over the crest. In the present case the sill was one-fifth of the expected maximum depth. Another factor, of less importance, is the depth of water that is to be flowing over the sill. The height of the sill is modified directly by the depth of the tail-water because the greater this depth, the thicker the overflowing jet will become in passing through it, and a higher sill is necessitated to cover the range of the thickened jet. A sill, or a series of blocks with no tail-water over them, is practically useless as the jet is merely sprayed into the air almost

as high as the original water surface, and falls to the river bed again, gaining nearly all its original velocity. There must be tail-water to float it away, so to speak, after the jet has been deflected from the river bed. Even then, the high velocity is merely transferred to the surface and may erode the banks, and is undesirable.

If any type of block sill is used on this apron, entire dependence is placed on it for stopping the velocity, and a small break starting anywhere would endanger the entire structure. Logs coming over the spillway would be carried down by the high-velocity stream to strike directly against the sill. The proposed dentated sill was 10 ft high and 30 ft wide, involving a large quantity of concrete and form work in its construction. The anchorage would have been difficult and expensive. Furthermore, a royalty would have had to be paid for using this type of sill. With these inherent disadvantages to the use of a block sill, investigation of other devices for dissipating the energy of the overflow was started.

Hydraulic-Jump Apron

Level Apron.—The feasibility of using the phenomenon of the hydraulic jump for energy dissipation was studied first. The dentated sill was removed from the apron. As stated before, this apron was low in the tail-water, being at Elevation 60, and the depths over it at all stages of discharge were apparently too great to allow the hydraulic jump to form, and high velocities persisted along the bottom for a considerable distance down stream. This can be seen in Fig. 2 which shows the distribution of velocities when the tail-water is at its natural depth.

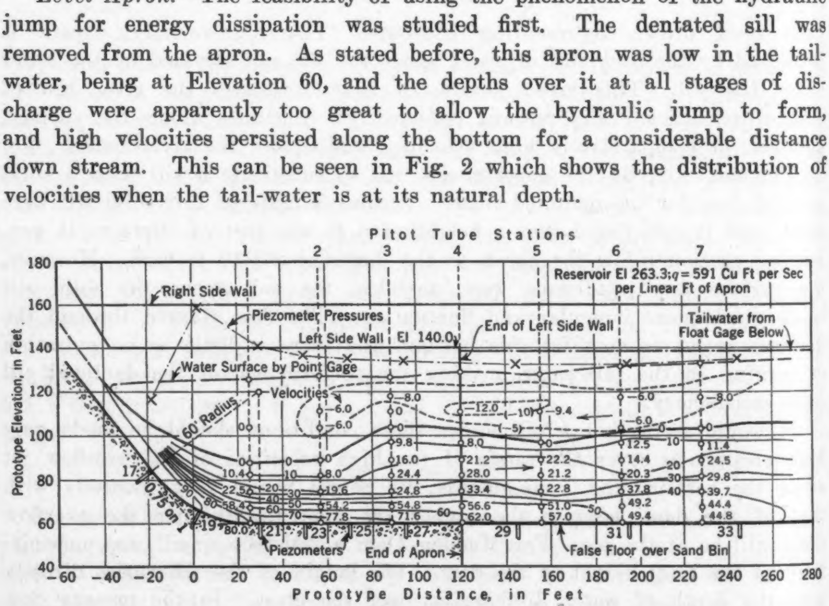

FIG. 2.—VELOCITY CONTOURS FOR FLOW OVER LEVEL APRON, WITH DEEP OR NATURAL TAIL-WATER

It was found that if the tail-water was lowered, the flow over the crest of the dam being kept constant, a certain point was reached at which the surface of the water became very turbulent and rose more or less abruptly from the toe of the dam, thence flowing off in a quiescent state. In other words, an apparently good hydraulic jump formation was taking place.

The tail-water was adjusted to force the beginning of this jump as far as possible back on the toe of the dam without "drowning" it. This adjustment was made for several flows, and the required tail-water noted. A plot of these elevations against the discharge is shown in Fig. 3 as

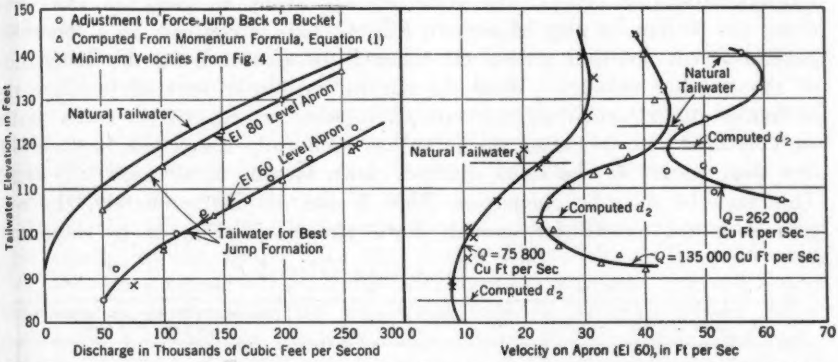

FIG. 3.—TAIL-WATER DEPTHS REQUIRED TO CAUSE THE BEST HYDRAULIC JUMP PERFORMANCE ON LEVEL APRONS AT DIFFERENT ELEVATIONS

FIG. 4.—VELOCITIES AT VARIOUS DEPTHS OF TAIL-WATER FOR THE SAME FLOW ON A LEVEL APRON

circle points. A check made with a Pitot tube showed that such adjustments of the tail-water were producing the maximum retardation of the velocities along the bottom. A Pitot tube was placed, and remained fixed, at a position within the jump and just off the floor of the apron. The tail-water was then varied and the resultant velocities, as indicated by the Pitot tube, were recorded. These velocities have been plotted in Fig. 4 for three different discharges. Although the velocities from the Pitot tube in this position are not the average velocities of the sections, it is thought that the curves thus obtained, show a relative energy dissipation for the variation in tail-water depths. Note that there is a decided peak in the curves that gives a definite tail-water depth to obtain a minimum resultant velocity. These peak points are plotted on Fig. 3, represented by the crosses, and check closely the first adjustments. Computation of the depths required to form the hydraulic jump, from the formula,

$$d_2 = \sqrt{\frac{d^2_1}{4} + \frac{2V^2_1\, d_1}{q}} - \frac{d_1}{2} \qquad (1)$$

shows close agreement. These computed points are plotted as triangles in Fig. 3; V_1, or the average velocity before the jump, was obtained from Pitot measurements, and,

$$d_1 = \frac{q}{V_1} \qquad (2)$$

was computed, in which $q = C\, d_c^{\frac{3}{2}}$. (In the notation of this paper an effort has been made to conform with "Symbols for Hydraulics,"[6] compiled by a

[6] A S A—Z 10b—1929.

Committee of the American Standards Association, with Society representation, and approved by the Association in 1929).

It is seen, then, that the natural tail-water at the dam site affords too great a depth over this apron to allow the jump to form. In fact, at natural tail-water depths, the velocities are about as great as can exist along the bottom, as may be seen in Fig. 4. This is contrary to a somewhat general acceptance that a deep tail-water is an assurance for the dissipation of the overflow velocity. With the natural tail-water lowered to allow the hydraulic jump formation, the rate of velocity retardation is much faster and the final velocity, after the jump has been fully completed, is decidedly less than occurs at the same distance down stream under high tail-water. This may be seen by comparing Figs. 2 and 5. Unfortunately, the section does not extend far enough down stream, in Fig. 5, to show this

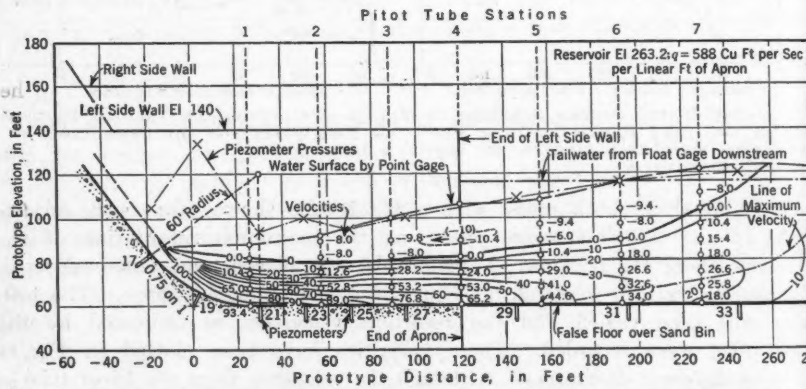

FIG. 5.—VELOCITY CONTOURS FOR FLOW OVER A LEVEL APRON, WITH TAIL-WATER LOWERED TO PERMIT THE FORMATION OF AN HYDRAULIC JUMP

final point of complete recovery of the jump where the velocity has been reduced to a minimum and is practically constant across the section.

The surface rise in the jump for a flow of this magnitude is very gradual, and the jump action extends for a considerable distance down stream, although the point of beginning is forced back on the toe of the dam. A back flow or roller occurs along the rise. The higher velocities hug closely to the floor until gradually retarded. The flow lines shown in Fig. 5 are typical although for smaller discharges the surface rise is more abrupt and, consequently, the length of the jump is shorter.

The depths required for jump formation, obtained by lowering the tail-water, are considerably less than the actual existent depths over an apron placed at this low level of Elevation 60. Since the natural tail-water at the dam site is fixed, it was necessary to raise the apron to a higher elevation in order to lessen the depths over it so that the jump would not be "drowned out." This alteration was made to the model, the apron being raised 20 ft to Elevation 80 and the foregoing tests were repeated. The jump formed well up on this apron at all flows

although indications were that it could be raised 2 or 3 ft higher, which would cause a closer coincidence of the natural tail-water depths to those required to produce the best hydraulic jump. This is illustrated in Fig. 3.

Sloped Apron.—Although a close coincidence may be obtained there would be only one flow, theoretically, where the natural tail-water depth over the level apron would give exactly the depth required for the jump. It was thought that, if the apron was inclined, the jump could move up or down the slope until the depth required was encountered, regardless of the variations in the tail-water. Furthermore, a considerable saving in concrete would result in this case because the sloped apron would more nearly follow the original rock contour.

From the aforementioned experiments of the hydraulic jump on the level apron, it was found that the depths which caused the minimum floor velocities agreed closely with the theoretical depths for the jump formation as computed by the momentum formula. For the sloped apron the computation becomes involved, due to the necessity of considering the gravity component of the water in the jump and nothing satisfactory could be developed from attempts to adapt the formula to this condition. A formula that included this weight component would show that a greater depth is required for the jump, which simply means that it would form farther down the slope where this greater depth is encountered. Even with this knowledge there is no method of predicting a satisfactory tail-water elevation, one that would start the jump well up on the apron at any flow. The difficulty lies in the fact that the datum plane is indeterminate, above which the required vertical depths are to be assumed.

The theoretical position of the jump can be computed but it would not be known whether this position would start the jump well back against the face of the dam and yet not so far back as to "drown it out." Unless the jump starts well up on the apron, full use is not made of the apron length. The calculated position might start the jump some distance down the slope away from the face of the dam and, therefore, for equal protection this length must be added to the pavement down stream.

Apparently, the solution was a case of experimental "cut and try" and a series of tests was made on aprons of various elevations and slopes. Fig. 6 shows tests on aprons of two different slopes. The apron first tested (Apron *A*) had a slope of 1 on 6.6, beginning at Elevation 84. The tail-water below this apron was adjusted so that the jump formed well up on the apron, starting back against the face of the dam. This condition was the first desired prerequisite. It is seen, however, that the required jump depths are less than the actual depths resulting from natural tail-water, indicating that the apron should be raised. This was done until it was found that an apron beginning at about Elevation 97 gave the best average results. The tail-water over this apron was adjusted until the jump was forced back against the face of the dam, as was the first procedure in the tests on each apron. A check was then made of this adjustment by measuring the velocities under various depths of tail-water.

A Pitot tube was placed, and remained fixed, at a point just off the floor and near the end of the apron. The tail-water was raised and lowered until a depth was found which caused the least velocity to be registered.

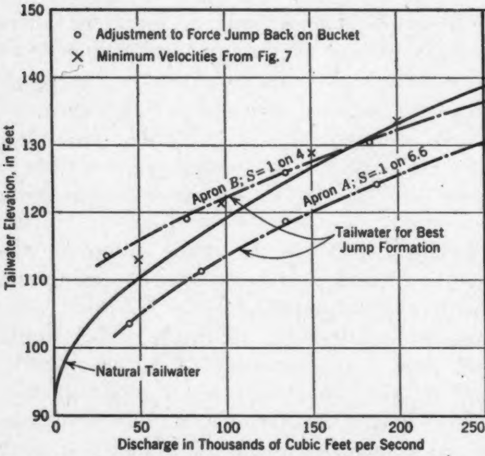

FIG. 6.—TAIL-WATER DEPTHS REQUIRED TO CAUSE THE BEST HYDRAULIC JUMP FORMATION ON SLOPED APRONS AT DIFFERENT ELEVATIONS

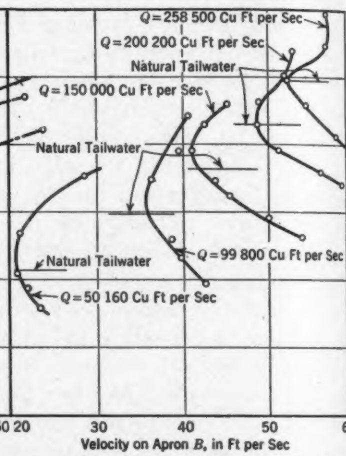

FIG. 7.—VELOCITIES AT VARIOUS DEPTHS OF TAIL-WATER FOR THE SAME FLOW ON A SLOPED APRON

The resulting velocity curves, obtained from the Pitot tube readings, are shown in Fig. 7. The peak points from these curves, indicating the tail-water required for minimum velocities, check closely the tail-water adjustments first made. The latter have been plotted, in Fig. 6, against the discharge to show the range of depths required for the best hydraulic jump formation.

Different slopes were tried, but apparently a slope of 1 on 4 was about as steep as could be used and still retain a good jump formation. On steeper slopes the overflowing jet cut under the tail-water with little surface disturbance or consequent loss of energy. From a study of the curves shown in Fig. 6, indications are that if Apron *A* is raised, a closer average coincidence of the jump depths with the natural tail-water would be obtained than in the case with the raised Apron *B*. This leads to the thought that the shape of the jump curve may be varied by changing the slope of the apron. This curve could then be made to conform closely with any given natural tail-water curve by adopting a proper slope to the apron.

The steepest apron that could be used would result in the greatest economy in this case and since it was found that the depths of the jump over Apron *B*, beginning at Elevation 97.3, averaged closely with the actual tail-water depths, this apron was adopted as satisfactory for the final design of the model. From later studies of the results of these tests, the final construction plans were changed to lengthen the spillway apron by 30 ft and to lower the elevation of beginning of the slope 3 ft.

Both these alterations were seen to be beneficial. The jump curve for Apron *B* shown in Fig. 6 lies above the natural tail-water rating curve at the lower discharges, indicating that the natural depths are not quite sufficient to force the jump well up on the apron at these flows. It may be supposed that, since the tail-water depth is fixed by the rating curve, if the apron is lowered the depth over it is increased and more nearly approaches that required for the jump.

Lowering the apron will improve the jump formation at the lower stages of flow, which occur more frequently. At the higher discharges the required depth of jump will fall slightly below the natural tail-water, but a factor of safety is so obtained in case the rating curve (which was extrapolated for higher flows) is less, or in case the natural tail-water is lowered at the dam site, by the erosion of the gravel beds down stream. Incidentally, the model tests showed that the tail-water could be lowered as much as 20 ft without the jump being swept off the apron.

Extending the apron 30 ft farther down stream would furnish greater protection to the river bed and, as the slope was also flattened, the concrete saved at the upper end was used to make the extension down stream. The apron as built at Madden Dam had a slope of 1 on 4.3, beginning at a point tangent to the bucket (radius, 60 ft) at Elevation 94.3 and extending from this point 158.15 ft down stream with a still 5.7 ft high, having a sloping face at the end.

Deflector Sill.—At all but the smallest flows the length of the hydraulic jump, or the point down stream where full recovery takes place, is beyond the end of the proposed apron. Theoretically the apron or protective pavement should extend down stream to a point where the bottom velocities have been reduced by the jump to an extent which will not erode the foundation rock. It was found from the tests that a small turn-up sill at the end of the apron would lift the velocities off the river bed below, but would not destroy the functioning of the jump. The thought was that if the sill was too high, impact would result and the stream would be deflected upward, thus "blocking out" the static pressure of the tail-water necessary for the jump. The tail-water might even be swept out between the toe of the dam and the sill. The sill should act only as a deflector, to lift the sheet of water just high enough to clear the bottom, allowing it to continue down stream where the velocities are dissipated in the normal action of the jump.

Expected Jump Performance.—The action of this sill and the functioning of the hydraulic jump on a sloped apron are shown in Fig. 8, which is a composite of some of the results obtained from the model tests on the final design of the sloped apron, under natural tail-water conditions. Water-surface curves, taken by point-gage measurements, are drawn for five different rates of flow, covering the entire range of discharge. Velocities through each cross-section were measured with a Pitot tube. Velocity contours for the lowest flow only (but which are typical) have been drawn in Fig. 8. Energy gradients, representing the maximum velocity heads

above the water surface at various stations, are shown for each discharge. The loss in energy through the jump is illustrated graphically, being greater and quicker for the smaller flows. Velocities in the overflowing sheets

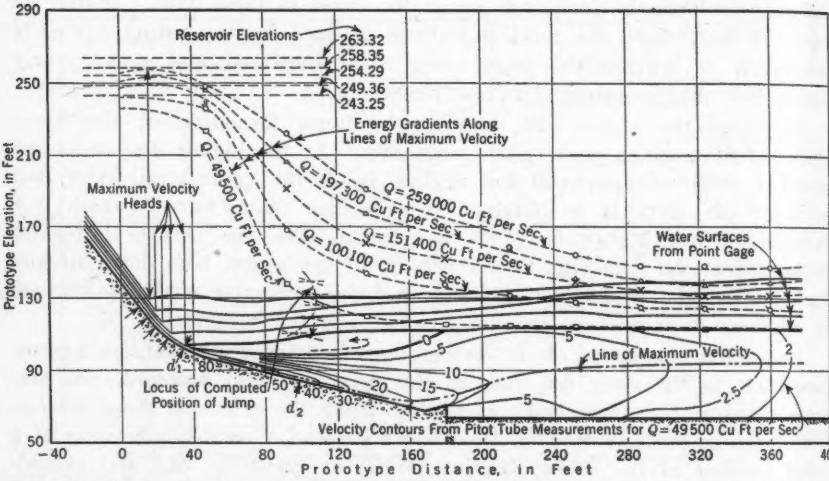

FIG. 8.—HYDRAULIC RELATIONS OF THE FLOW OVER A MODEL OF THE MADDEN SPILLWAY, REPRESENTING NATURAL CONDITIONS FOR THE ENTIRE RANGE OF DISCHARGE

just before they strike the tail-water were taken with a Pitot tube and were found to agree closely with Equation (2) when d_1 was measured with a point gage. These velocities, especially at the higher flows, amounted to almost the theoretical values since the surfaces of the model were painted and sand papered to a very smooth finish. On the prototype structure greater loss in friction may be expected. Provision was made at Madden Dam to measure this loss by installing Pitot tubes on the spillway toe. A factor of safety is obtained, however, by keeping the friction losses on the model low.

The velocity, V_1, and the depth, d_1, before the jump being measured in each case, the depth required for the hydraulic jump was computed from Equation (1). These depths, or d_2, have been plotted, in Fig. 8, parallel to the slope of the apron and at the points where they are crossed by the tail-water elevations, projected up stream, a line is drawn as the locus of the theoretical position of the jump. It is perhaps significant that these computed positions lie fairly close together near the center of the apron.

As on a level apron, the highest velocities lie along the floor, becoming gradually less toward the top with a reverse flow or back roll on the surface (see Fig. 5). At the beginning of the jump, pressures on the apron, as recorded from piezometers, lie slightly below the water surface, measured with a point gage. This is probably due to the air content of the water. Pressure recorded on the up-stream face of the sill is little greater than the static pressure which indicates only slight impact. Pres-

sure recorded in the curve of the bucket at the toe of the dam is higher than the static depth of water, which may be attributed to the centrifugal force of the stream.

Retardation of the velocity of the overflowing sheet is not instantaneous; the highest velocities persist along the floor until gradually reduced and the jump is not fully recovered until some distance beyond the end of the apron. At a flow of 49 500 cu ft per sec (see Fig 8), although the measured velocity of the water at the toe of the dam before entering the jump, was 89.3 ft per sec, the highest velocity obtained at the end of the apron was 16.8 ft per sec. This remaining velocity, however, is lifted clear of the river bed below by the deflector sill and about 40 ft farther down stream, at which point the jump action is practically completed, the mean velocity of the cross-section was less than 7 ft per sec. At the higher discharges the action is analogous but a greater length down stream is required to complete the full recovery.

There is a similarity of action between the hydraulic jumps occurring on a level floor and on an inclined floor. The rate of velocity retardation on Apron *B*, Fig. 6, seems to be as fast as that along the level apron until the end of the slope is reached, after which the retardation is much slower and the velocities persist farther down stream than is the case when the jump occurs on a level apron. Apparently, the stream is taken down to depths too great to allow the jump to form completely. The steeper the slope of the apron the less perfect is the jump formation. The slope of 1 on 4, adopted here for concrete economy, is about the steepest that can be used and still obtain a satisfactory jump formation.

The jump performance, as discussed in connection with Fig. 8, is typical for the total range of discharge over the spillway. At any flow the beginning of the jump is forced back against the face of the dam and although the distance down stream required for full recovery increases with the discharge and extends beyond the end of the apron, the bottom velocities are lifted free from the foundation rock by the deflector sill so that no erosion results. It was found to be feasible, therefore, to shorten the length of the theoretical spillway apron considerably by placing a sill of the proper dimensions at the end. This apron need only cover the start of the hydraulic jump where the first impact and consequent turmoil occur.

Fig. 9 shows the characteristic relations of the hydraulic jump when it is made to form on a sloped apron at the toe of an over-flow dam. The curves have been obtained from results of these model studies, but it is believed they can be applied, for design approximations, to any installation roughly similar to Madden Dam. The functions represented by the curves are directly proportional to the depth over the crest, and the height of the fall, and the values may be used for any dam where the ratio of these two factors is known.

A datum line is assumed, usually at low flow, or no flow, tail-water elevation. A sufficient length of the apron must be above this elevation in

order that the beginning of the jump (which moves up stream at each increase in discharge) may occur on a flat slope. A good jump formation will obtain with the point of beginning on the curved surface of the bucket, but not if it is forced back on the steep slope of the down-stream

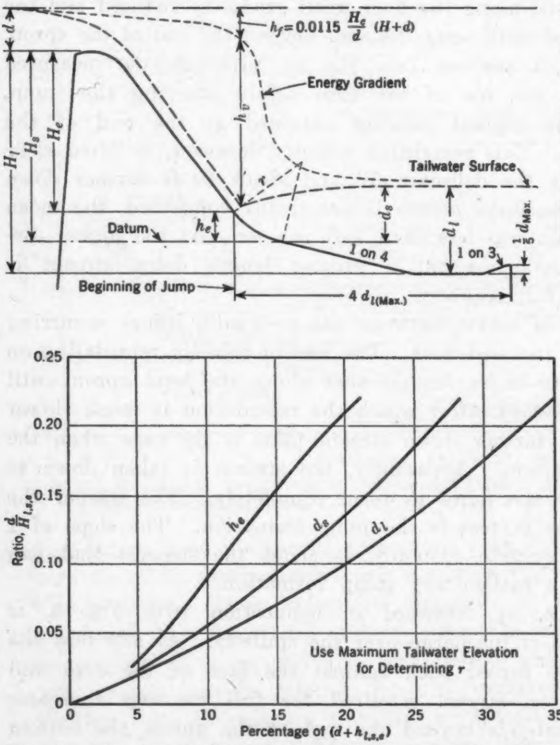

FIG. 9.—HYDRAULIC RELATIONS REQUIRED TO HOLD JUMP FORMATIONS ON THE SLOPED APRON AT TOE OF OVERFLOW DAMS

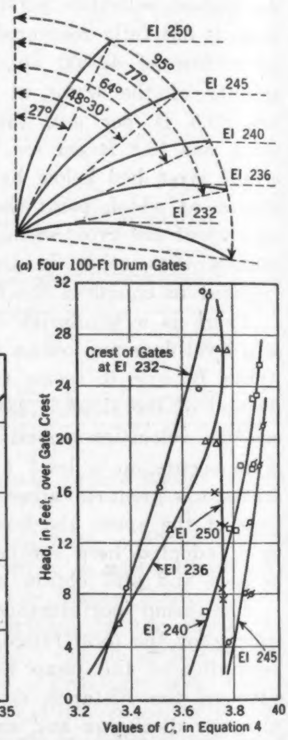

FIG. 10.—COEFFICIENTS OF DISCHARGE OVER CREST. DRUM-GATES AT VARIOUS POSITIONS

face of the dam. The jump should begin as far back as possible, however, in order to utilize the full length of the apron. For the maximum expected flow it should begin at the point of curvature of the spillway bucket from the face of the dam.

The maximum expected tail-water is then used to determine the maximum, d_s, and the maximum, h_e, which locate the point of curvature for the bucket with the radius, r, as shown in Fig. 9. The slope of the apron should not exceed 1 on 4 and should be carried down at least to a depth represented by the function, d_l.

Friction Loss at Toe.—Measurements of the velocity through the overflow jet at the toe of the dam were made with a Pitot tube. They were taken at the same station at the toe, but for various depths of water over the crest. The difference between the indicated near velocity head

from the Pitot tube and the total theoretical velocity head (which is the vertical distance between water surfaces) was taken as the head loss due to friction. The values thus obtained were found to be averaged closely by a curve of the form:

$$h_f = 0.0115 \frac{H}{d_c} (H + d_c) \quad \ldots\ldots\ldots\ldots (3)$$

in which H = the height of the dam, from apex of crest to toe; and, d_c = the depth of water over the crest, from reservoir level to apex of crest. Equation (3) should hold true for friction loss at the toe of any model dam similar to that of Madden Dam. This model was covered with galvanized iron, enameled and sand-papered very smooth. Indications from the tests are that the value, $C = 0.0115$, increases with the roughness of the surface and would be 0.0135 for models covered with galvanized iron which had not been painted or sand-papered. For concrete dams $C = 0.02$ was found to average closely the values obtained from the prototype tests, discussed subsequently.

Other Devices.—Variations in the design of the apron were tried but none of them proved successful enough to be incorporated or to warrant a change in the sloped apron layout. Some of the alterations tested were: (*a*) The sloped apron built in steps; (*b*) a channel or pocket below the lip of the bucket cut into the sloped apron; (*c*) a high spillway bucket designed with a vertical drop to the apron paving[7]; and (*d*) transverse channels or dentates cut into the high bucket (later used at Tygart Dam).[8] Most of these alterations were attempted with the idea of saving concrete, as the apron was very thick at the toe of the dam after it had been raised to the necessary height to cause the hydraulic jump formation.

Later, this heavy apron was included in computing the stability of the dam, greatly increasing the factor of safety against earthquake stresses and also against sliding. The simplicity of design, the ruggedness of performance, and the adaptability to conditions at Madden Dam gave preference to the use of the plain sloped apron, with a small deflector sill, to obtain the hydraulic jump; so that investigations of other devices for dissipating the energy of the overflow were discontinued.

Other Factors of Spillway Design

Right Bank Excavation.—In addition to the development of the spillway apron, model tests were made to determine other features of the spillway design. The proper treatment of the excavation along the right bank proved difficult of a theoretical analysis. Immediately below the dam site, the river bends abruptly to the left and the original contours of the right bank protruded across the flow from the last spillway gate. The original design contemplated leaving this hillside in place and paving the steep 1 on 1 slope for protection against the spillway flow. The

[7] *Engineering News-Record,* March 14, 1935, p. 376.

[8] *The Military Engineer,* September-October, 1936, p. 331.

model tests showed that the hillside was too steep for the water to "ride" and, consequently, the flow slipped down into a concentrated jet at the foot of the slope and caused disastrous erosion down stream from the pavement. Alterations were tried in which this slope was made flatter, but little success was attained in attempting to make the water flow in a uniform sheet along a slope of almost any inclination, however slight. A warped surface probably could have been developed that would carry its share of the discharge but still no provision would be made for checking the velocity of the water. A sill placed along the top was not very satisfactory as the tail-water over it became less and less up the slope and the flow was sprayed into the air, only to fall back on the unprotected bank below the apron.

It was finally decided that the safest and most positive results would be attained by cutting into the bank and extending the apron across the full width of the spillway. It was found possible to level off gradually and raise the down-stream end of the sloped apron toward this right bank (see Figs. 12 and 13). However, the apron was kept down under the tail-water so that the depth of water over it was sufficient to form the hydraulic jump at any flow.

Tail-Race Walls.—Another problem that arose from the effects of this bend in the river below the dam was the checking of the eddy or reverse flow which naturally formed along the left bank. This eddy is accentuated by the water surface in the hydraulic jump on the spillway apron which is at a lower elevation than the tail-water below the power house. This condition creates a difference in pressure and a cross-flow. The effect is detrimental to the functioning of the jump, causing an unbalanced condition resulting in concentrations of velocity. This reverse flow also tends to deposit sand and gravel in the draft-tube openings and to set up surgings in the tail-race which might hinder the speed regulation of the turbines.

To remedy the condition a division wall was placed down stream from the power house to separate the spillway and turbine tail-race. Theoretically, this wall should be high enough to prevent overflow into the spillway at any discharge and should extend down stream until the water-surface rise in hydraulic jumps levels out. It was found by experimentation that if the height of the wall followed the actual surface rise in the jump, the over-pour on to the top of the jump caused no harm. Accordingly, the wall was built at this point 12 ft below the elevation of the maximum expected tail-water, with a resultant saving in concrete. On the other side of the river, the hydraulic jump is contained by paving along the bank.

Erosion Below Spillway Apron.—Prediction of the nature of the erosion likely to occur below the spillway apron was possible from the model studies. As previously stated, the original gravel deposits in the river bed were represented on the model by building up the contours in sand (see Fig. 1). No attempt was made to graduate this sand to the scale of the model as

the objective was to reduce the velocities to a point where no erosion of the foundation rock itself would occur. Thus, erosion tests on the model were qualitative rather than quantitative. It was found that for the smaller flows, up to a quantity representing 50 000 cu ft per sec, the sand contours were scarcely disturbed, but for larger flows the over-burden was washed from the right bank and river bottom below the apron and deposited as a bar across the main river channel. As successive floods free from sediment are passed over the spillway, this bar may be expected to move farther down stream and expose the bed-rock. The maximum expected velocity to which this rock may be subjected is about 25 ft per sec moving along the surface without direct impact. From field tests in which specimens of the foundation rock were subjected to high velocity jets, no erosion is expected at this velocity.

Drum-Gate Operation.—The proper operation of the spillway drum-gates was quickly seen from the model performance. The operation is necessarily fixed by the method adopted for dissipating the energy of the overflow. One of the first prerequisites of a good hydraulic jump formation is to have the water enter the jump in a uniform jet of equal velocity across its entire width. This is readily accomplished at Madden Dam by opening the four spillway drum-gates simultaneously. The design of these gates allows for spilling over the top at any partial opening. If one, two, or three gates are opened, the jump on the apron is broken up by the inpour of the adjacent tail-water. The water below a closed gate will be at a higher elevation than the surface rise of the water in the jump and a cross-flow will result. Although the jump is not completely "drowned out" it was found that concentrations of flow were set up which greatly aggravated the erosion below the apron. Except for the small flows, or for very short periods of time, it would be dangerous to open the gates singly.

Discharge of Drum-Gates.—In the crest of the model, the drum-gates were reproduced to scale. With the gates at several different positions of closure and the pond at various elevations, coefficients of discharge curves were determined by plotting values of C in the formula:

$$Q = C\,L\,H^{1.5} \qquad \text{.........................(4)}$$

against the head on the gate crest (see Fig. 10). In the tests the head was measured, in the customary manner, from the highest point on the gate to the water surface up stream where the velocity of approach was negligible.

From these curves the maximum discharge of the spillway was determined, and was found to be in excess of the design assumptions.

The quantity of water that would discharge over the top of the gates in the fully raised position was found. This information was desired as it may prove advantageous to throttle the discharge by raising the gates after the peak of a flood has passed, in order to protect shipping in the canal. Passage is interrupted when the flow exceeds 50 000 cu ft per sec at

the point where the river and the canal join down stream. This was one of the factors considered in determining the height of the drum-gates.

Nappes and Pressures on the Crest.—At different gate-openings, with various heads of water flowing over the crest of the gates, the outlines of the surface curves were taken by means of a point gage. The pressures along the crest were obtained by piezometers. Fig. 11 with Table 1

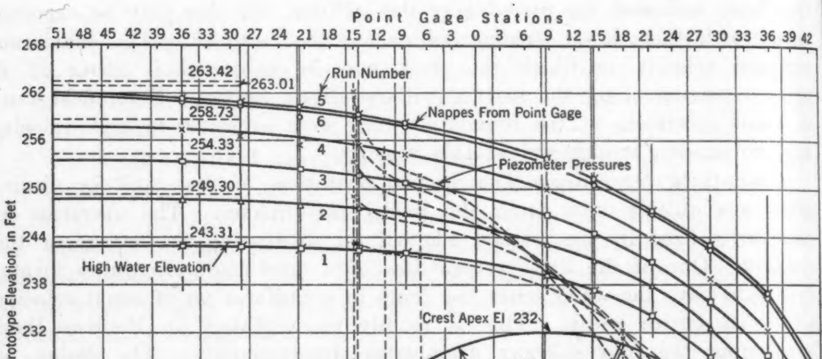

FIG. 11.—NAPPES AND PRESSURES ON SPILLWAY FOR VARIOUS FLOWS

TABLE 1.—TEST RUNS CORRESPONDING TO FIG. 11

Run No. (1)	Ratio of flow, Q, in cubic feet per second (2)	Head on crest, in feet (3)	Run No (1)	Ratio of flow, Q, in cubic feet per second (2)	Head on crest, in feet (3)
1.........	50 200	11.31	4.........	204 200	26.73
2.........	99 800	17.30	5.........	258 400	31.72
3.........	150 000	22.33	6.........	253 500	31.01

shows a typical set of curves taken for a condition representing the drum-gates fully opened. It will be noted that a vacuum was recorded on the down-stream face of the crest for all the flows tested. Preliminary analysis of the proposed crest by the flow-net method showed this vacuum to exist, and its effect was recognized in the design of the crest. A check of this analysis was made by reproducing the assumed conditions on the model. A very close agreement was found in the pressures, both negative and positive, along the crest. However, the water-surface curve obtained on the model lay considerably below the water surface assumed for the computation, from the apex of the crest down stream.

Spillway Retaining Walls.—In order to check the height and stability of end retaining walls of the spillway, nappes and pressures against them were measured at various positions of the gates. The maximum possible case was taken with the forebay at its highest elevation and the gates opening. It was found that as originally designed, the wall needed to be raised slightly and the outline changed in order to follow more closely the shape taken by the overflowing nappe.

At one position of the partly opened gate, this nappe rose nearly to the top of the wall for practically its entire length. Nevertheless, the velocity of the water was such that a negligible pressure was recorded against the wall, except at the very bottom where the jet has finally spread enough to cause impact. This impact amounted roughly to a pressure equal to the depth of water flowing over the crest of the gate.

Other Features.—Other features of the design (not previously discussed herein), that were developed from the model tests included: The proper location of sluice-ways through the spillway section; the shape of the deflecting lip over the sluice-way exits; and the location of the control float intake, at the drum-gate. The sluice-ways, which were rectangular (5 ft 8 in. by 10 ft 0 in. high) discharged into the hydraulic jump on the spillway apron at Elevation 89.67, about the level of low tail-water (see Fig. 13). No serious interference with the jump formation was noted in the tests when the sluice-ways were located between the spillway gates; that is, below the spillway piers. The original plans were changed to move them to this location from the first position which was midway between the piers. Over the exit, where the sluice-way comes through the down-stream face of the spillway section, a deflector lip, the proper dimensions of which were determined by experimentation, was placed to relieve back pressure that may be caused by spillway flow. A check was made to be sure that no vacuum developed in the sluice-ways during the flow over the spillway exits.

Considerable data were obtained on hydraulic jump performance, both on level and sloping aprons, which included velocity contours, water surfaces, and pressures through the jump at various flows. Comparative data on the performance of sills of various heights and shapes were also secured. Performance data were obtained on the action of the other devices previously mentioned for dissipating the energy of the overflow at the toe of ogee dams. Motion and still pictures were taken of nearly every set-up that was tested on the model.

Conclusions Supported by Model Tests

Conclusions pertaining to model tests of Madden Dam may be stated on the basis of the foregoing data, as follows:

(1) The hydraulic jump is very effective in dissipating the energy of the overflow at the toe of ogee dams, the resultant velocities at the end of the jump being little greater than those normally present in the river channel during a given flow, if no dam were present.

(2) The jump can be formed and controlled best by placing a sloped apron down stream, tangent to the bucket at the toe. It will move up or down the slope with variations in tail-water due to different discharge.

(3) The elevation of the apron must be placed relative to the tail-water so that the depths over it will start the jump precisely at the toe of the dam, for all flows, in order that the total length of the apron may be utilized.

(4) The slope of the apron should not be steeper than 1 on 4. A change in the slope alters the shape of the required jump-depth curve. By adopting a proper slope to the apron a close coincidence may be obtained with the natural tail-water depths throughout the entire range of discharges.

(5) The length of the jump, or the distance down stream required for full recovery, varies as its magnitude, increasing with the discharge.

(6) The highest velocities through the jump occur near the floor until they are gradually retarded

(7) The length of the spillway apron then should cover the range of velocity retardation until a point is reached where the velocities are low enough not to cause the river bed to scour down stream.

(8) This apron length may be shortened by placing a sill, with a gradually sloping face, at the end to deflect the floor velocities just free from the river bed below, until they are retarded in the natural action of the jump.

(9) The jump should be protected or enclosed by vertical side walls to a height, at least, which follows the rise in the water surface through the jump, in order to prevent "washing out" by the cross-flow of the adjacent quiet tail-water. It is more desirable to have the side walls high enough to exclude the adjacent tail-water entirely. The walls should extend down stream to the point of full recovery where the water surface reaches the same elevation as that of the adjacent tail-water.

(10) The water entering the jump should be of equal depth and velocity across the entire width; or, in other words, the discharge over the dam crest should be uniform. This is accomplished at Madden Dam, without the use of training walls for each gate, by simultaneously lowering the four drum-gates to the same opening.

(11) The hydraulic jump method is much more advantageous than any other device for dissipating the energy of the overflow below dams. The design is simpler and more rugged when the energy is dissipated by the impact of water on water. The spillway is free from obstructions that may be damaged by logs and other debris coming over the dam. Sills, piers, etc., to break the force of the water are expensive and difficult to reinforce and anchor and are always liable to damage. A break at any point creates a high velocity concentration, which starts erosion that may undermine the entire structure. Difficulties of repair are likely to prove costly as these sills, etc., are usually under a considerable depth of tail-water; if they are not, they are practically useless in dissipating the energy.

Conclusions (1) to (11) have been reached from the tests on the design of Madden Dam. The apron is placed, by experimentation, so that

a balanced effective hydraulic jump occurs well up on the apron at any given flow over the spillway. This is accomplished more easily at Madden Dam than elsewhere, perhaps, as the foundation rock was of sufficient depth below the natural tail-water so that an apron of the proper elevation and slope could be constructed. It is thought, however, that from the results of these tests, principles may be established that will allow the adaptation, at other projects, of this type of apron for obtaining the hydraulic jump. The tendency in later designs of overflow dams has been to use a sloped apron for obtaining the hydraulic jump as a means of dissipating the energy at the toe. Similar aprons were later used at the Norris Dam,[9] and at the Hamilton Dam, in Texas. Preliminary designs for the Marshall Ford Dam, in Texas, and the Friant and Kennett Dams, in California, contemplate the use of this type of apron.

The effectiveness of the hydraulic jump and the possibilities of a sloped apron as a means for controlling it were fully demonstrated by the model studies of Madden Dam.

PART II.—PROTOTYPE TESTS

Introduction

In order to check the model predictions and also to obtain hydraulic data on a full-sized structure, test apparatus was installed at Madden Dam similar to that used on the model. Piezometers were placed along the spillway crest of one gate in the sides of the spillway piers, and down the training wall. Provision was made to measure the water surface over the crest. Piezometers and Pitot tubes were placed at the toe of the spillway, to measure the head lost in friction, and along the length of the apron to measure the velocity lost through the jump. A gaging station is situated just down stream from the dam for obtaining the quantity of water flowing.

The concrete apron built at the toe of the spillway is constructed on a slope of 1 on 4.3, taking off tangent to the curved bucket at the toe and terminating in a small sloped-face deflector sill. The dimensions are given in Figs. 12 and 13 which show a general layout of the dam. As stated in Part I this apron was adopted after extensive model tests had been made to determine the best method for dissipating the energy of the overflow.

The largest flow that has been passed over the spillway to date (1937) has been about 32 000 cu ft per sec. Complete model data for a direct comparison are not available at this relatively low discharge, but the results obtained are interesting and are indicative of the performance to be expected for greater flows.

Nappes and Pressures Over the Crest

The water-surface profiles of the flow over the spillway crest were obtained by dropping a heavy steel plumb-bob, attached to a steel tape,

[9] *Civil Engineering*, April, 1935.

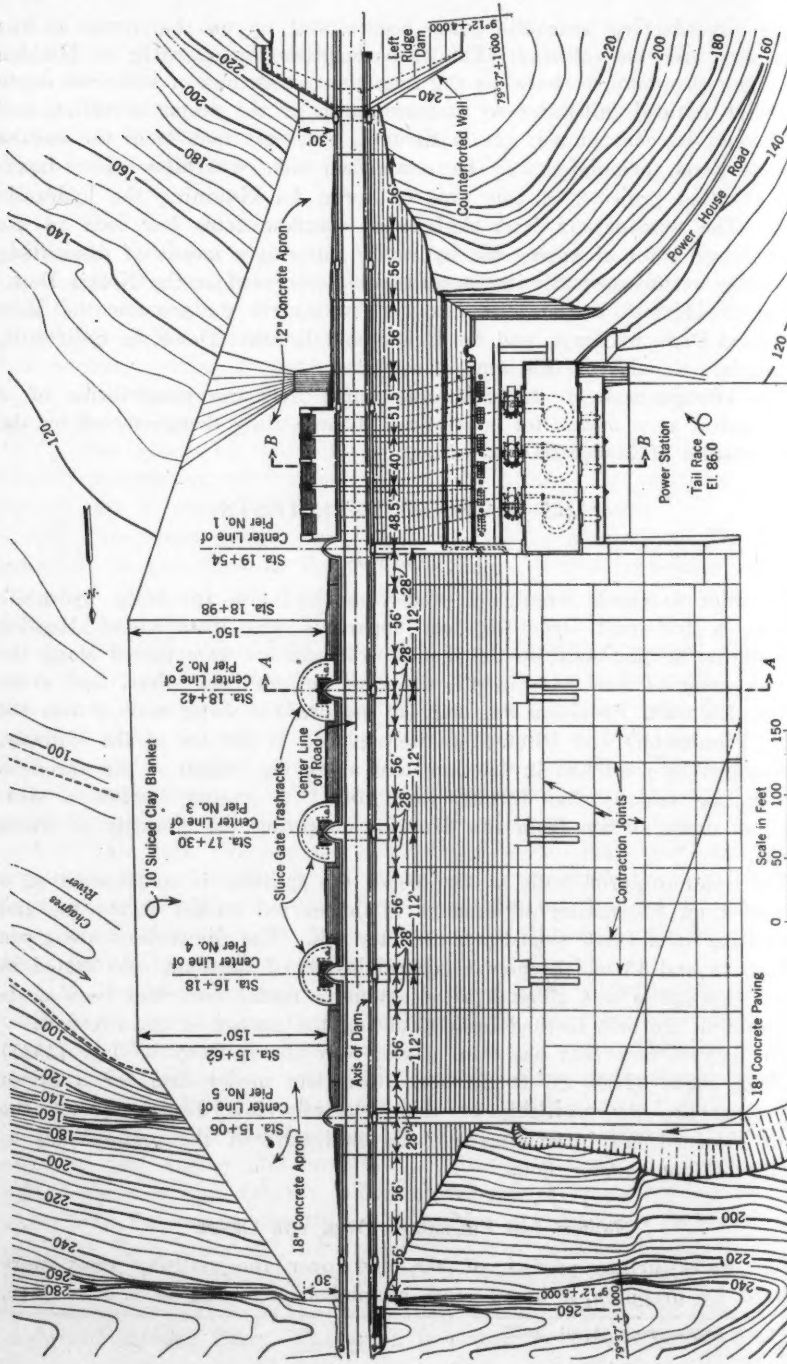

FIG. 12.—PLAN OF MADDEN DAM

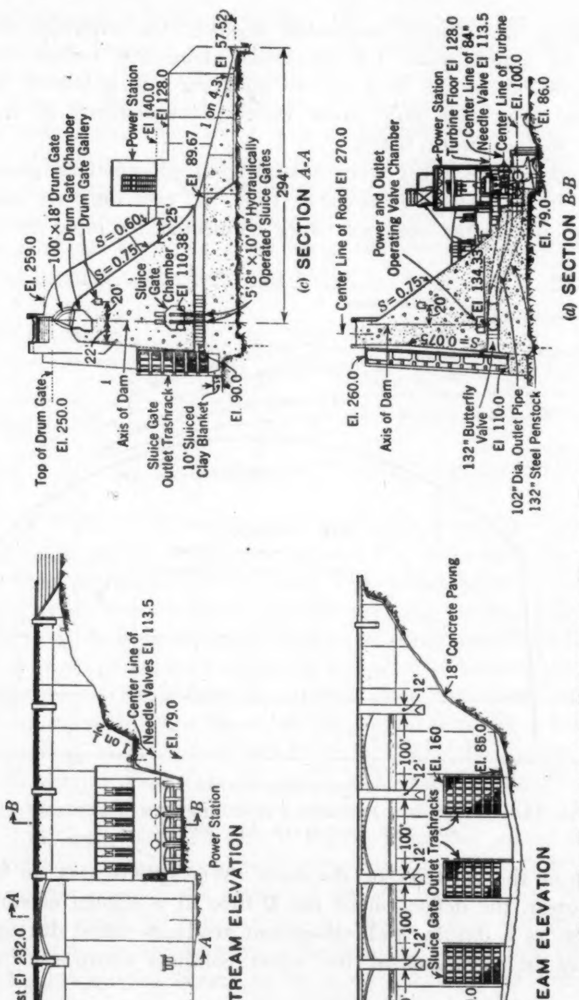

FIG. 13.—SECTIONS AND ELEVATIONS, MADDEN DAM

from a platform which was suspended beneath the archway of the first drum-gate. At approximate 4-ft intervals along the center line of the gate, holes were cut in the floor of the platform and a known bench-mark was established near each hole, from which measurements of the distance to the water surface were taken.

Pressures along the crest were taken from piezometers placed in the concrete and in the steel drum-gate itself, at positions as indicated in Fig. 14. The piezometer openings were connected by pipes to a mercury

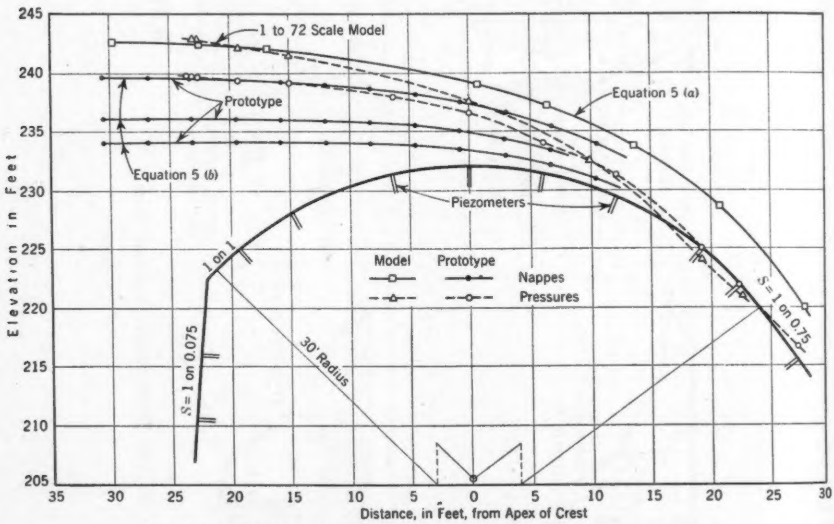

FIG. 14.—NAPPES AND PRESSURES OVER THE SPILLWAY CREST, FOR THE PROTOTYPE AND MODEL

U-tube in one of the galleries of the dam. When there was no water flowing over the crest, the deflection of the **U**-tube at a known elevation of the lake, was taken as a datum, and subsequent readings taken during the tests were translated in elevations of the water columns above each piezometer opening.

It will be noted that the pressures as recorded by the piezometers on the up-stream face of the dam lie above the measured water surface, indicating impact. The pressures drop off rapidly along the crest due to the centrifugal force, reaching a negative pressure at a point on the downstream side, showing a tendency of the jet to leap the crest. The model tests showed that this tendency increased at higher heads. The vacuum thus produced was taken into account in the stability design of the crest, as its existence was early recognized.

Fig. 14 shows the nappes taken over the prototype crest for three heads. Three other intermediate heads were also taken for this position of the gate fully opened (lowered), and lie consistently between the ones shown. The piezometer pressures are plotted for the higher profile only, in order

to avoid confusion in Fig. 14, but the pressures for the lower heads fall in a similar manner below the water-surface nappe in each case. A water-surface curve line of pressures taken on the model, is also shown in Fig. 14. This is the lowest head on which complete data were taken for the model tests. However, there is a remarkable consistency in the curves indicating similarity of performance. Nappes and pressures were also taken with the gate in three different positions of closure.

Many trials were made to obtain an equation for these overflow nappes. For water surfaces over the crest when the drum-gates were fully opened (lowered) the most consistent formulas that could be derived were as follows:

For the model:

$$-y = (0.164\,H - 0.692)\,1.223^{xH^{-0.5}} \qquad (5a)$$

and, for the prototype:

$$-y = (0.0733\,H - 0.206)\,1.266^{xH^{-0.5}} \qquad (5b)$$

in which: $\pm x$ and $-y$ are co-ordinates of a point on the water surface, with the Y-axis at the up-stream face of the dam and the X-axis as the horizontal line of the reservoir level. The model formula (Equation (5*a*)) was derived from five measured nappes, at heads corresponding to a range from 11 ft to 31 ft over the crest. The prototype formula (Equation (5*b*)), was derived from points on five curves taken up to a maximum head of 7.78 ft over the crest. Each formula showed close agreement with its respective test points, within the range of the points actually taken.

When applied to a head of 7.78 ft (which was the highest head attained over the prototype spillway), Equation (5*a*) showed close agreement with the actual measured water surface on the prototype On the other hand, Equation (5*b*) gave a profile much higher than the actual measured water surface over the model for a head corresponding to 11.3 ft over the crest (which was the lowest head measured on the model). In other words, the model formula could be extrapolated to check the prototype measurements, but the prototype formula, when extrapolated, would not check the model nappes. Equation (5*a*) seems to be more accurate for design approximations, until further tests are taken to check Equation (5*b*).

A row of piezometers was placed along the face of one of the end spillway piers. In general, the pressures recorded on these piezometers fell slightly below the profile of the water surface There was a noticeable disturbance at the end piers of the spillway. The slight drop below static pressure which was recorded along this end pier may be due to the tendency of the flow into the gate to leave the side of the pier as it comes in from the dead end of the spillway. This disturbance could be eliminated, perhaps, by extending the end piers a distance up stream, although in most instances this is not practical. At the intermediate piers, when adjacent gates are opened, there is no disturbance, the flow being symmetrical and uniform on both sides of the pier.

Piezometers were located along the drum-gate just beneath the downstream tip of the top skin-plate. These piezometers, however, did not indicate vacuum for any of the flows attained to date. With the drum-gate fully opened (at which position it conforms with the crest curve), the vacuum (see Fig. 14) noticed at the higher heads occurs at a point down stream from the tip of the gate. At any of the partly raised positions of the gate, there is no indication of a vacuum on the under side, the overflowing sheet of water being quite sufficiently aerated at the ends, through the space behind the square end piers. Piezometers were installed in the left retaining wall of the spillway, but the flows have not yet been high enough to obtain readings from them.

Spillway Discharge

Simultaneously, with the observations being taken at Madden Dam, stream-gage measurements were made at the permanent station about ½ mile down stream. The readings were made with a current meter at 1-ft depth, the mean velocity being obtained by using a correction factor of 0.9. This factor (0.9) was established for the station after a long period of observations in which complete traverses were made. Due to the high velocity through the gaging section of the river it was not possible to make complete traverses during the larger flows, and the 0.9 factor used may be in considerable error for the higher discharges. It is apparent that to measure the discharge in a degree of accuracy comparable with the model tests special apparatus must be used to hold the current meter in the high velocity water so that a complete traverse of the section can be made.

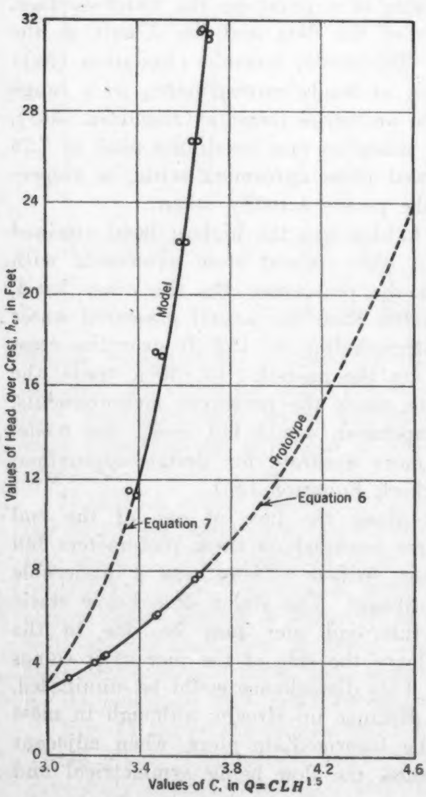

FIG. 15.—COEFFICIENTS OF DISCHARGE

The results as obtained may prove interesting, however. A curve of coefficients of discharge was computed, using the simultaneous discharges taken from a rating curve of the gaging station established from readings taken during these tests. The equation of the coefficient curve for drum-gates fully opened, was obtained from the test points and showed that the coefficient of discharge was a function of the

head as well as of the crest shape:

$$C = 2.44 \ h_c^{0.20} \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (6)$$

Fig. 15 shows a plotting of this coefficient curve, extrapolated by Equation (6) for heads higher than those tested, for comparison with the model coefficient curve, similarly extrapolated downward by the formula:

$$C = 2.69 \ h_c^{0.094} \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (7)$$

to cover the range of the lower prototype test heads. At the maximum prototype test head of 7.78 ft over the crest, the indicated discharge is about 13% greater than that predicted from the model tests. Some of this flow may be due to the possible error in the stream gaging. There is an apparent definite discrepancy, however, as the two curves have marked different characteristics.

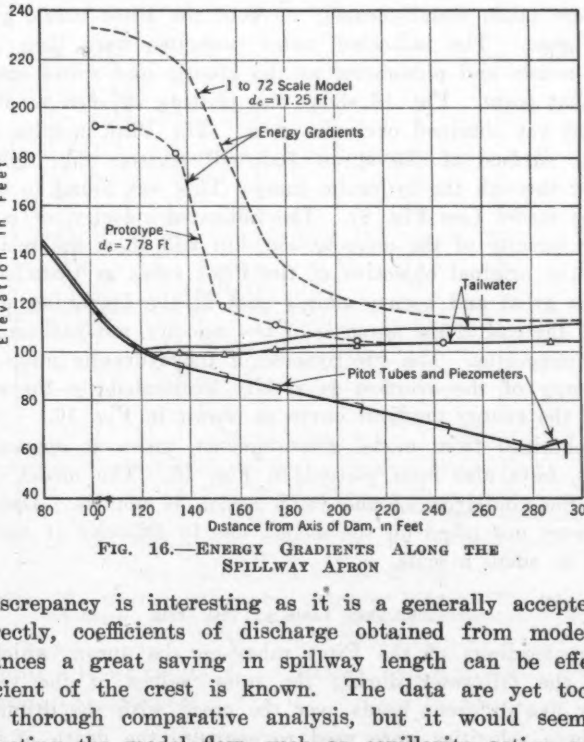

FIG. 16.—ENERGY GRADIENTS ALONG THE SPILLWAY APRON

This discrepancy is interesting as it is a generally accepted procedure to use directly, coefficients of discharge obtained from model tests. In some instances a great saving in spillway length can be effected if the true coefficient of the crest is known. The data are yet too meager to attempt a thorough comparative analysis, but it would seem that some factors affecting the model flow, such as capillarity action and viscosity, have not been given full cognizance.

VELOCITIES ALONG SPILLWAY APRON

During construction a number of fixed Pitot tube nozzles were installed along the spillway apron at positions indicated in Fig. 16. The Pitot tubes

consisted of a plate steel pedestal, 4 in. wide by 12 in. long, the plate being bent to a stream-line shape, welded, and filled in with concrete. The bottom of the pedestals were embedded and anchored in the concrete and protruded 14 in. for some to 20 in. for others above the floor of the spillway apron. A piece of 1-in. pipe projected 4 in. from the up-stream edge of the pedestal at heights above the floor varying from 3 in. for those pedestals above tail-water, to 12 in. and 18 in. for those pedestals covered with tail-water.

These projecting pipes or nozzles were connected by 1-in, pipes to a manifold and pressure gage in the basement of the power station. Below each nozzle, but offset 2 ft to one side, were piezometers, opening flush with the concrete surface of the apron. These piezometer openings were conducted through separate pipes to a manifold and pressure gage in the power station. The pressure gages were accurately calibrated previous to and during the tests. With any given flow over the crest, a number of readings were taken simultaneously on both the Pitot nozzle gage and the piezometer gage. The indicated water pressures were then plotted over each Pitot nozzle and piezometer as the kinetic and static energy, respectively, at that point. Fig. 16 shows the plotting of such readings for the highest head yet obtained over the crest. The Pitot nozzles are situated just off the surface of the apron and will register the highest velocity of the water through the hydraulic jump. This was found to be true from tests on the model (see Fig. 8). The indicated velocity, of course, is not the average velocity of the cross-section, but will show the relative change, which was the original objective of the Pitot tubes as installed. The loss in energy is great and occurs mainly just at the beginning of the jump. The flow off the end of the apron is of low velocity, not sufficient to register on the test apparatus. The effectiveness of the hydraulic jump in dissipating the energy of the overflow is vividly illustrated by the sudden drop obtained in the energy gradient curve as shown in Fig. 16.

Curves obtained from model measurements, taken at approximately the same points, have also been plotted in Fig. 16. The model discharge is higher but the similarity of the jump action is notable. Data for lower discharges were not taken on the model, due to difficulty of accurate measurement on so small a scale.

Friction Loss at the Toe

From the readings of the Pitot tubes on the apron, which were not covered by the tail-water during the tests, values of the velocity were obtained for five different heads over the crest, with the drum-gates fully opened. These velocities were used to compute the depth of the stream, the rate, Q, having been obtained from stream gaging below the dam. The difference between the velocity head as indicated by the Pitot tube readings and the theoretic velocity head (which is the difference between water surfaces) was taken as the loss in head due to friction on the overflowing sheet of water. These rather crude Pitot tubes will not, of course,

measure the true mean velocity of the stream, but a discussion of the friction loss as so indicated by them may prove interesting. It was found that the average of the five readings obtained at different heads over the crest could be expressed by an equation of the following form:

$$h_f = \frac{f_n\, H\, (H + d_c)}{d^q_c} \quad \text{.....................(8)}$$

in which h_f = the friction head loss at a given station on the toe of the dam; f_n = the friction coefficient depending on the roughness of the surface; q = the exponent of the depth, expressing a relation to the friction loss; H = the height of the dam, from crest apex to station on toe (factor involving the length of the channel, which is practically constant for all overflow dams); d_c = the depth of water over the crest, from reservoir elevation to crest apex; and $H + d_c$ = an expression of the velocity.

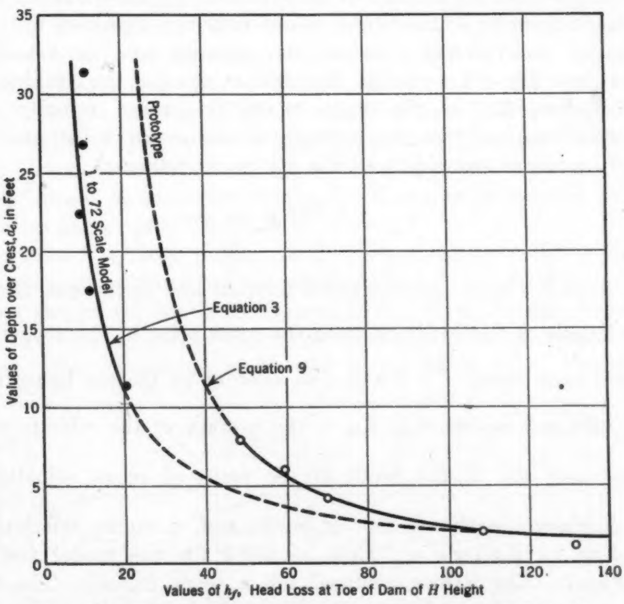

FIG. 17.—CURVES EXPRESSING FRICTION LOSS AT TOE OF OVERFLOW DAMS, AS OBTAINED FROM OBSERVATIONS ON MODEL AND PROTOTYPE

By averaging all test points, $f_n = 0.00934$ and $q = 0.623$ were obtained so that Equation (8) becomes:

$$h_f = \frac{0.00934\, H\, (H + d_c)}{d^{0.623}} \quad \text{.....................(9)}$$

A curve of Equation (9) with the test points shown, has been plotted in Fig. 17. Extrapolation is made for comparison with a curve similarly obtained from the model tests (Equation (3)).

The prototype curve can be made to fit the model-test points very closely by changing the coefficient, $f_n = 0.00934$ to $f_n = 0.00374$, or 0.4 of the prototype coefficient. The friction loss, then, occurring at the toe of an overflow dam of any size, whether model or prototype, may be expressed by Equation (8) when f_n is a coefficient of friction depending on the roughness of the surface, varying from $f_n = 0.00374$ for the painted galvanized iron surface of the model, to $f_n = 0.00934$ for the concrete surface of the prototype dam. The formula is expressed in prototype values and the resulting friction loss, h_f, must be divided by the scale of the model (in this case, 72) to obtain the actual value in terms of the model dimensions.

In attempting to check the theory that this difference in energy loss might be due to the roughness of the surface, numerous attempts were made to apply the Chezy formula to the test points. Results varied, depending on assumptions, as to the mean velocity and the mean hydraulic radius. A constant, n, could not be obtained by substitution in Kutter's or Manning's formula. Consistent values were not obtained until the mean velocity, V_m, was assumed to be the average of the velocity at the crest apex (see Fig. 11 and Fig. 14) and at the toe, and taking the mean hydraulic radius, R_m, at the depth where this mean velocity occurs. A formula was obtained in the exponential form which satisfied both the model and prototype test points within 8% ±, as follows:

$$V_m = \frac{1.233}{n} R_m^{0.45} S^{0.57} \quad \text{.....................(10)}$$

in which $S = \frac{h_f}{L}$; h_f = the measured friction loss at the toe, from Fig. 17; L = the length of the spillway from the crest apex to the toe (the velocity at the crest apex being $\frac{3\,q}{2\,d_c}$; and, the velocity at the toe being taken from the Pitot tube measurements); V_m = the average of the velocity at the crest and at the toe; R_m = the depth at the point of mean velocity $= \frac{q}{V_m}$; q = the discharge per linear foot of crest; and, n = the friction coefficient corresponding to Kutter's n (taken as 0.009 for the model and 0.016 for the prototype). The values assumed for n seem logical. The model was enameled and sand-papered to a very smooth finish. Form marks left at the horizontal construction joints made the down-stream face of the prototype dam unusually rough. It is indicated, then, that Equation (10) could be used for computing resulting velocities at the toe of dams, by substituting the proper value of n for the surface and solving in the same manner as that used to derive the equation.

When and if further tests are made on the prototype at heads comparable to the model data, the coefficient of the Pitot tubes as used could be obtained from tests on a model, resulting in more accurate data for determination of the friction loss. The comparisons that have been attempted show the possibilities.

Drum-Gate Operation

The correct procedure for operating the drum-gates was derived from the model tests, as previously discussed. It was found that in order to obtain the proper functioning of the hydraulic jump, all spillway drum-gates should be opened to the same amount. To verify this model prediction, an attempt was made to measure the resulting erosion below the spillway apron, with various combinations of drum-gate openings.

A schedule of tests was run in which, first only one gate was opened fully and allowed to spill for 3 hr, then all four gates were opened partly so that the same quantity of water was spilled as was passed through the one gate. The four gates were then allowed to spill for 3 hr. This same procedure was repeated with two gates fully opened and then with the four gates partly opened to pass the same quantity of flow; and, similarly, with three gates. After each of these settings, soundings of the river bottom were taken in the tail-race below the apron for the purpose of comparison. The change in the profile of the river gravel was so slight, however, that no definitely conclusive comparison could be reached. There seemed to be only a shifting of the deposits, with little movement down stream, at any of the combinations tested. The gravel was moved at one place and piled up at another. There were indications, however, that the flow, with only one or two gates opened, tended to form concentrations and to erode channels that might be excessive if the discharges were greater than those attained during these tests.

Fig. 18 shows a typical profile of the gravel deposits below the apron before and after all spillages in the aforementioned series of tests. It will be noted that the gravel has been smoothed off, but no erosion occurs below the lip of the sill, which is the vital point. Quite a depth of gravel remained over the foundation rock. At higher flows more severe erosion

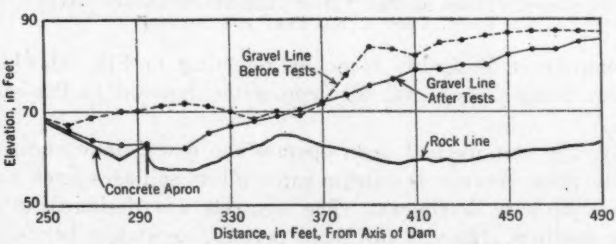

Fig. 18.—Typical Erosion Below Apron

of the over-burden may be expected, but it was found from the model tests that the sill eliminated the possibilities of scour undermining the apron, at any flow.

Fig. 19 is a view taken from the spillway bridge showing the flow from one gate only. Note that the adjacent tail-water crowds the beginning of the jump, causing the flow to concentrate toward the retaining wall. This concentration produced excessive gravel erosion below the apron.

A more positive indication of the effects of one gate operation was obtained from readings taken on the Pitot tubes along the apron, which

were situated below Gate No. 1. If this gate were opened alone there was a marked increase in the velocity along the apron as compared with the velocity existing if all four gates are opened partly to pass the same flow.

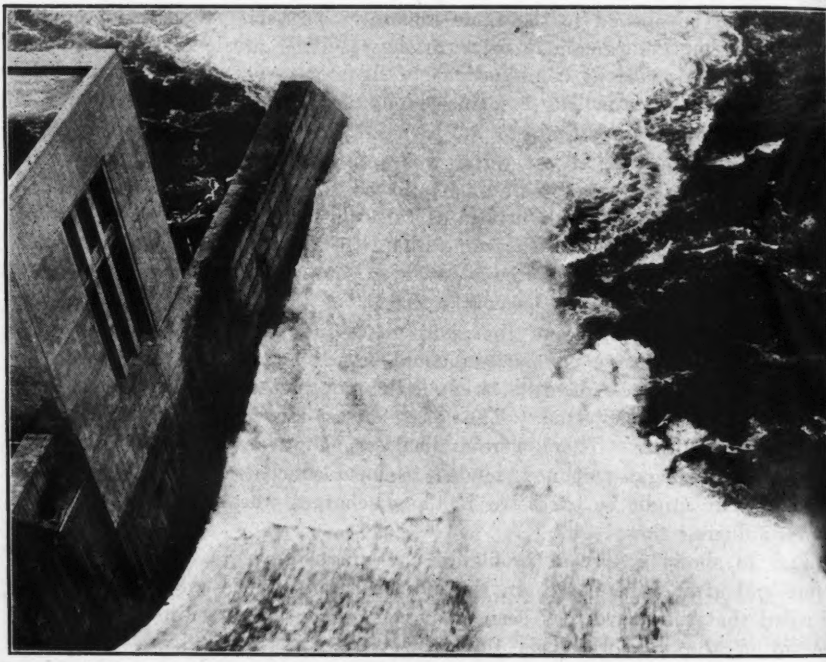

FIG. 19.—VIEW OF SPILLWAY BRIDGE, SHOWING INTERFERENCE WITH HYDRAULIC JUMP ON APRON, CAUSED BY THE TAIL-WATER BELOW CLOSED GATES; FLOW, 7 500 CUBIC FEET PER SECOND

A visual comparison is readily made by referring to Fig. 20 which shows the same flow being passed over all drum-gates, lowered to the same opening.

Obviously, the best method is to operate the gates simultaneously which can be easily done because the drum-gates allow spillages over the top in any position, without detriment. The controls are designed to hold the gate in any position. Except for short periods, or at low heads, the indications are that it would be dangerous to allow spillages from the gates opened individually.

CONCLUSIONS: HYDRAULIC JUMP PERFORMANCES

At the flows over Madden Dam spillway to date, the performance of the hydraulic jump has been consistent with the predictions made from the model tests. The depths furnished by the natural tail-water have caused the jump to begin just at the toe of the dam at any flow. Fig. 21 shows the hydraulic jump at the toe of the dam for the maximum flow attained to date, of 31 800 cu ft per sec. Fig. 20 shows the jump at a lower flow.

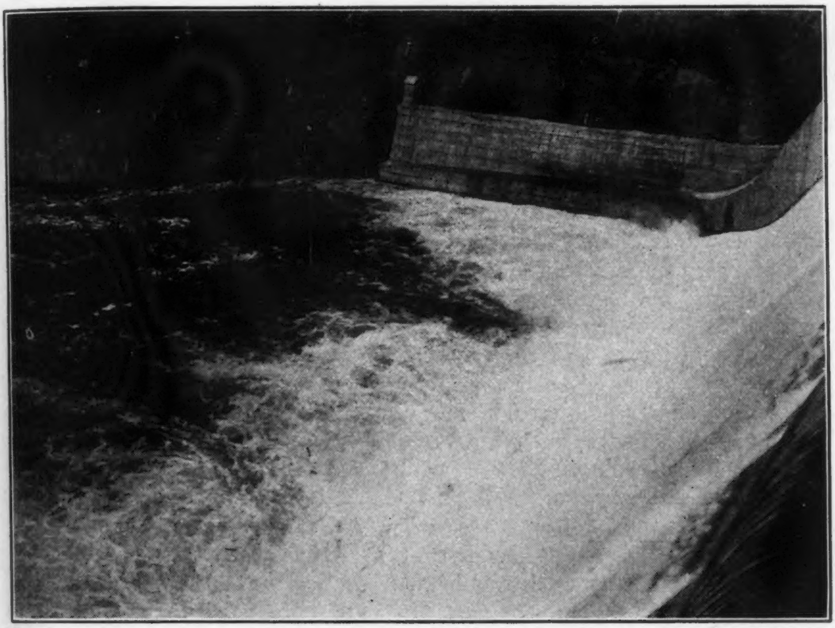

Fig. 20.—View from Spillway Bridge, Showing Hydraulic Jump on Apron; Flow, 7 600 Cubic Feet per Second

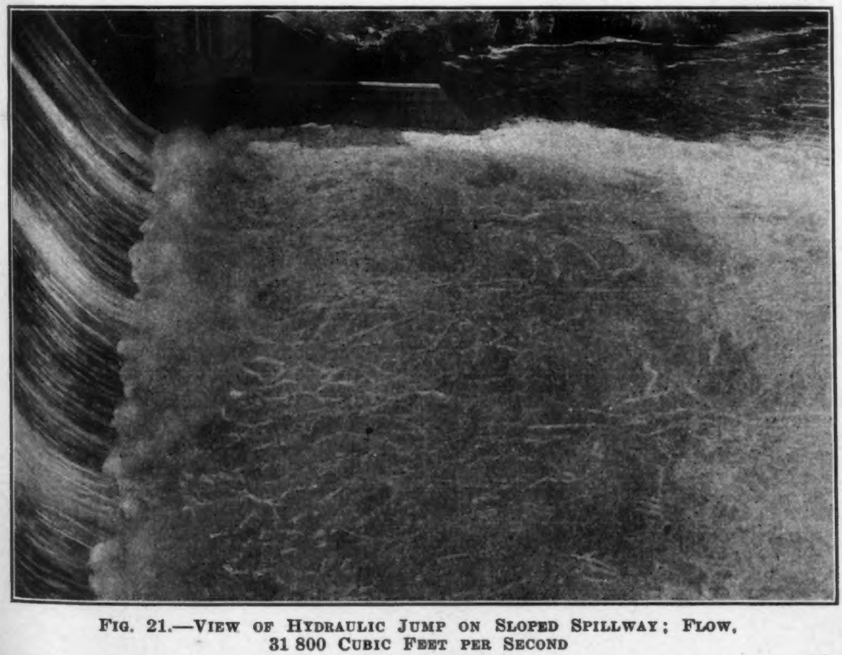

Fig. 21.—View of Hydraulic Jump on Sloped Spillway; Flow, 31 800 Cubic Feet per Second

The loss in energy in the jump has been almost instantaneous and the resultant erosion of the gravel deposits beyond the concrete apron, almost negligible (see Figs. 16 and 18).

It will be noted from Fig. 17, that the measured velocity at the toe was less on the prototype. This may be attributed to the difference in the roughness of the surface of the model and the concrete dam, as discussed previously.

For other comparative measurements it was found that the nappe curves taken over the spillway crest on the model could be used to predict the prototype nappes within a reasonable degree of accuracy. The comparative pressures on the crest seemed consistent. An apparent increase was obtained in the discharge over the crest of the prototype dam from that predicted from the model. All the model predictions were on the safe side, however.

The foregoing prototype test data were obtained at comparatively low flows over the spillway and there are no corresponding model data available for a direct comparison. Accurate and complete data were not taken on the model for very low heads over the crest for two reasons: (1) The model was built on a rather small scale (1:72) and very low quantities of discharge were difficult to measure; (2) the principal objective of the model experiments was to determine the best method to handle the larger and maximum flows safely, so that few data were obtained on flows less than a prototype equivalent of 50 000 cu ft per sec. For greater flows however, complete and accurate records were obtained at various increments of flow up to the maximum expected discharge of 260 000 cu ft per sec.

The comparisons which have been attempted herein were made from extrapolations, but it will not be until these tests are completed for higher discharges that a comprehensive study can be made. From what has been indicated, many valuable and interesting data could be developed by further testing, which, in comparison with the model data, would establish a more reliable criterion for model similitude.

Acknowledgments

The Madden Dam project was completed in 1935 under the administrative direction of Col. Julian L. Schley, Corps of Engineers, U. S. Army, M. Am. Soc. C. E., then Governor of The Panama Canal, assisted by Col. Clarence S. Ridley, Corps of Engineers, U. S. Army, then Engineer of Maintenance; and Lt. Col. William E. R. Covell, Corps of Engineers, U. S. Army, M. Am. Soc. C. E., then Assistant Engineer of Maintenance. Edward S. Randolph, M. Am. Soc. C. E., was first Designing Engineer and, later, Construction Engineer, on the project. The designs and specifications were prepared by the forces of the U. S. Bureau of Reclamation, in Denver, Colo., in collaboration with the Panama Canal organization. Raymond F. Walter, M. Am. Soc. C. E., Chief Engineer, and John L. Savage, M. Am. Soc. C. E., Chief Designing Engineer, of the U. S. Bureau of Reclamation, served as Consulting Engineers for the design and construction.

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PAPERS

READJUSTMENT OF TRIANGULATION DATUM

By Julius L. Speert,[1] Jun. Am. Soc. C. E.

Synopsis

The readjustment of the basic triangulation of the United States to the 1927 North American Datum created a problem to all organizations whose surveys were built on the basic system. Complete recomputation and readjustment being out of the question because of their prohibitive cost, the United States Geological Survey investigated methods of readjusting its earlier triangulation to fit the new datum by applying corrections to the old datum values. Since the corrections are usually very small, approximate methods of determination give reasonably precise results.

Several methods of readjustment have been developed. When the subordinate triangulation is tied to the new datum through a single base only, three methods are available: (1) Analytical determination of the new positions by applying formulas for translation, rotation, and linear scale change; (2) a graphical method for determining corrections by plotting lines of equal position changes; and (3) a mechanical method requiring an easily constructed transparent rotating guide. When the subordinate triangulation is tied to the new datum at three or more points, the new positions are determined graphically by plotting lines of equal position changes. The corrections to length and azimuth of the geodetic line are determined analytically by formulas, or graphically on a nomographic chart.

The methods herein described are applicable also to the adjustment of new triangulation.

Introduction

When the United States Coast and Geodetic Survey began its readjustment of the basic triangulation system of the United States in 1927, it created a problem of major importance to practically all other surveying organizations in the country. All triangulation of lesser importance that

Note.—Discussion on this paper will be closed in September, 1937, *Proceedings*.
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had been computed from, or based on, the arcs of the main plan, no longer fitted these arcs as they were shifted or changed by the readjustment. When the task of bringing all its accumulated triangulation data into agreement with the new 1927 North American Datum was undertaken by the United States Geological Survey, it was realized immediately that direct recomputation from the original field notes was out of the question, because neither time nor money would be available for the execution of such a huge project. To avoid the necessity of complete recomputation, various methods have been proposed and tried wherein the changes in the main control system have been studied, in order to ascertain what changes must be introduced into the subordinate triangulation to regain the harmony that had existed on the old datum, or to establish harmony where none had existed before. In dealing with changes of position, direction, and length, instead of with those quantities themselves, the magnitudes of the terms in the computations are kept so small that simplifications can be introduced readily without diminishing the accuracy of the final results.

Much of the old Geological Survey triangulation was originally adjusted as spurs from single bases. Many of these arcs have since been made parts of closed circuits by later ties to other arcs but, in most cases, the closing errors have not been distributed throughout the work. The adjustment of the closures of these circuits simultaneously with the readjustment to the 1927 North American Datum also was undertaken.

Wilson Method

The first rational attempt to readjust the old Geological Survey triangulation to the 1927 North American Datum by means of corrections to the previously computed values, was made by R. M. Wilson, M. Am. Soc. C. E., Chief of the Computing Section of the Survey. His method was fully and clearly described in a paper presented before the Federal Board of Surveys and Maps on April 11, 1933. The following is a brief summary of his method.

Many chains or networks of triangulation have been extended from single basic lines of higher order triangulation of the Coast and Geodetic Survey. In such a net, any change in the length, azimuth, or position of the base could be transferred to the remainder of the network without affecting any of the adjusted angles of the earlier figure adjustment within it. This being true, it is necessary only to determine the amount of change in the co-ordinates of the ends of the base, and the corresponding change in azimuth and length of the base. The entire system can be translated bodily to bring one end of the base into its new position; the system can then be rotated about this point to bring the base into its correct azimuth. If, finally, a uniform scale factor is applied to all lines of the system, the other end of the base can be brought into its correct position, and the entire system will be in harmony with the new datum. By means of formulas developed by Mr. Wilson, the translation, rotation, and scale

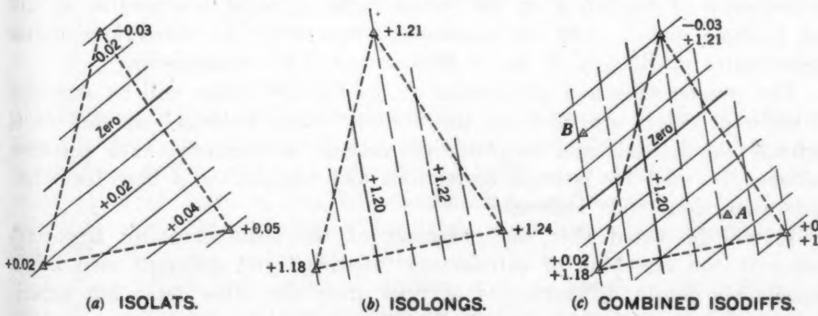
factors may be applied to all points in the triangulation net to determine their positions on the new datum, and all lines may be corrected by the uniform azimuth and scale changes.

This method is limited in its application to those systems of triangulation that are developed from a single base and have no other ties to the new datum. In many nets, however, other ties have been made, either at the time of the original survey, or subsequently. In such nets, the values obtained by this method would fit the original base perfectly, but would be likely to show discrepancies at the other tie points. For these conditions, other methods of treatment were found necessary.

"Isodiffs"

For those triangulation nets in which three or more ties to the new datum are available, a graphical method for the determination of the position corrections has been developed by C. L. Nelson, Assistant Topographic Engineer, U. S. Geological Survey.

On a scale map or tracing diagram of the triangulation net to be adjusted, all available ties to the new datum are indicated, and the proper corrections noted which are needed to bring the old values for latitude and longitude to the new datum. By interpolating along lines between the various ties, corrections may be obtained for intermediate points between the fixed tie-points. When enough of such values have been determined, it is possible to draw lines on the tracing representing lines of equal corrections, or of equal changes from the previous positions. This is very similar to drawing contours on a map when the elevations have been determined at a number of fixed points. It will be necessary to have one set of lines for latitudes and another set for longitudes. These lines have been named "isodiffs" (or lines of equal differences) and are also called



(*a*) ISOLATS. (*b*) ISOLONGS. (*c*) COMBINED ISODIFFS.

FIG. 1.

"isolats" and "isolongs," respectively. Fig. 1 illustrates the construction of the "isodiffs" for the relatively simple case of three tie-points.

When the number of ties is greater than three, straight-line interpolation between tie-points sometimes leads to inconsistencies. For instance, in the illustration in Fig. 2, if an attempt were made to interpolate along

both diagonals, different values might be obtained for the intersection. Under such conditions, it is advisable to assume a value for the intersection which would be a weighted mean of the values obtained from the respective diagonals, and to proceed with the interpolation from that point as if it were a tie-point.

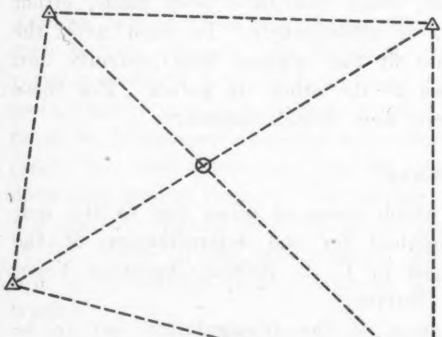


FIG. 2.

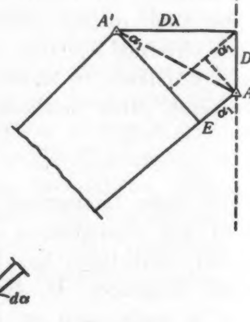


FIG. 3.

With the graph of "isodiffs" constructed on the tracing, it is necessary only to superimpose it on the plot of the triangulation plan, and the corrections to be applied to each station in the system may be read from its position on the graph, just as the elevation of any point on a contour map may be read from the contours, this being the purpose for which the graph is made.

For example, in Fig. 1(*c*), the corrections to be applied to the old co-ordinates of Station *A* in the system to be adjusted, are read as + 0.03 for latitude and + 1.22 for longitude. Similarly, the corrections to the co-ordinates of Station *B* are − 0.02 and + 1.18, respectively..

The construction and application of the "isodiff" chart will be expedited if different colors are used for the "isolats" and "isolongs," as well as to indicate the latitude and longitude corrections, respectively. For instance, red may be used for latitude corrections and "isolats," and blue for longitude corrections and "isolongs."

It will be noted that the judgment of the adjuster is an important factor in the sketching of satisfactory "isodiffs," and different men might conceivably obtain different sets of lines from the same data; but experience has shown that for practically all cases where three or more ties are available, excellent results can be obtained by this method. The "personal factor," furthermore, is not as serious a disadvantage as it might appear, since it is inherent in almost any method of adjustment in which a number of independent conditions must be satisfied. Even the application of the standard method of least squares is not altogether free from it.

The proper application of the "isodiff" graph gives corrections to be used in obtaining new values for the positions of the triangulation stations, but does not yield any new information concerning the lines connecting the stations. For this purpose, the following formulas have been developed by the writer.

New Formulas

When the required position corrections have been determined for the stations in a system and applied to the old values to bring them to the new datum, the lines connecting the stations are ready for readjustment. Although the inverse geodetic position computation could be used to determine the desired values for the lines on the new datum, sufficient precision can be obtained, at a considerable saving of time, by the use of approximate methods for the determination of the corrections to the old values.

To do this, consider a line connecting Triangulation Stations A and B in Fig. 3, with a length equal to s and azimuth at Station A equal to α_1. The corrections to the position of Station A (as determined by "isodiffs" or by any other means) are $d\phi_1$ and $d\lambda_1$ and to the position of Station B, $d\phi_2$ and $d\lambda_2$. If the line is translated by $-\,d\phi_2$ and $-\,d\lambda_2$, no change in its length or direction will take place. Then, the resulting relative changes in latitude and longitude at Station B will both be 0, and the changes at Station A will be $\delta\phi = d\phi_1 - d\phi_2$ and $\delta\lambda = d\lambda_1 - d\lambda_2$. These latter values represent the net change within the line, AB, or the relative change of one end with respect to the other, and are the values that cause whatever change there may be in the length or in the azimuth of the line.

Before they can be used in the computations, $\delta\phi$ and $\delta\lambda$ must be converted from seconds of arc to meters by the formulas:

$$D\phi = Q\ \delta\phi \quad \text{.......................} \quad (1a)$$

and,

$$D\gamma = R\ \delta\lambda \quad \text{.......................} \quad (1b)$$

The conversion factors, Q and R, represent the length on the earth's surface of 1″ of arc of latitude and longitude, respectively. The factor, Q, varies from 30.77 m at Latitude 25°, to 30.90 m at Latitude 50°, and a mean value of 30.83 m has been assumed as sufficiently precise for all points in the United States. The value of R varies more rapidly (see Table 1).

If $D\phi$ and $D\lambda$ are plotted at Station A to an enlarged scale, Point A' (Fig. 3), represents the new position of Station A with respect to Station B (to the distorted scale), and the total displacement of the station is represented by AA'. If this is projected on AB, then AE represents the linear change in AB (to the scale of the plot) and $A'E$ is a measure of the rotation of the line. The angular rotation at Station B (to bring BA to BA') $= -\,d\alpha = \dfrac{\overline{A'E}}{\overline{A'B}\sin 1''}$, approximately, $= \dfrac{\overline{A'E}}{s \sin 1''}$, approximately;

TABLE 1.—Values of R

Latitude, ϕ, in degrees (1)	Length, R, on the earth's surface, of 1 second of longitude, in meters (2)	Latitude, ϕ, in degrees (1)	Length, R, on the earth's surface, of 1 second of longitude, in meters (2)	Latitude, ϕ, in degrees (1)	Length, R, on the earth's surface, of 1 second of longitude, in meters (2)	Latitude, ϕ, in degrees (1)	Length, R, on the earth's surface, of 1 second of longitude, in meters (2)	Latitude, ϕ, in degrees (1)	Length, R, on the earth's surface, of 1 second of longitude, in meters (2)
25	28.04	30	26.80	35	25.36	40	23.72	45	21.90
26	27.81	31	26.53	36	25.05	41	23.37	46	21.52
27	27.57	32	26.25	37	24.73	42	23.01	47	21.13
28	27.32	33	25.96	38	24.40	43	22.65	48	20.73
29	27.07	34	25.66	39	24.06	44	22.28	49	20.33
..	.....	..	.....	..	.....	..	.....	50	19.92

TABLE 2.—Values of Factors C and K

Factor C (1)	log s (2)	Factor K (3)	Factor C (1)	log s (2)	Factor K (3)	Factor C (1)	log s (2)	Factor K (3)	Factor C (1)	log s (2)	Factor K (3)
43.4	4.000	20.61	33.4	4.114	15.86	23.4	4.269	11.11	13.6	4.504	6.46
43.2	4.002	20.52	33.2	4.117	15.77	23.2	4.272	11.02	13.4	4.511	6.36
43.0	4.004	20.42	33.0	4.119	15.67	23.0	4.276	10.92	13.2	4.517	6.27
42.8	4.006	20.33	32.8	4.122	15.58	22.8	4.280	10.83	13.0	4.524	6.17
42.6	4.008	20.23	32.6	4.125	15.48	22.6	4.283	10.73	12.8	4.531	6.08
42.4	4.010	20.14	32.4	4.127	15.39	22.4	4.287	10.64	12.6	4.537	5.98
42.2	4.012	20.04	32.2	4.130	15.29	22.2	4.291	10.54	12.4	4.544	5.89
42.0	4.015	19.95	32.0	4.133	15.20	22.0	4.295	10.45	12.2	4.551	5.79
41.8	4.017	19.85	31.8	4.135	15.10	21.8	4.299	10.35	12.0	4.559	5.70
41.6	4.019	19.76	31.6	4.138	15.01	21.6	4.303	10.26	11.8	4.566	5.60
41.4	4.021	19.66	31.4	4.141	14.91	21.4	4.307	10.16	11.6	4.573	5.51
41.2	4.023	19.57	31.2	4.144	14.82	21.2	4.311	10.07	11.4	4.581	5.41
41.0	4.025	19.47	31.0	4.146	14.72	21.0	4.316	9.97	11.2	4.589	5.32
40.8	4.027	19.38	30.8	4.149	14.63	20.8	4.320	9.88	11.0	4.596	5.22
40.6	4.029	19.28	30.6	4.152	14.53	20.6	4.324	9.78	10.8	4.604	5.13
40.4	4.031	19.19	30.4	4.155	14.44	20.4	4.328	9.69	10.6	4.612	5.03
40.2	4.033	19.09	30.2	4.158	14.34	20.2	4.332	9.59	10.4	4.621	4.94
40.0	4.036	19.00	30.0	4.161	14.25	20.0	4.337	9.50	10.2	4.629	4.84
39.8	4.038	18.90	29.8	4.163	14.15	19.8	4.341	9.40	10.0	4.637	4.75
39.6	4.040	18.81	29.6	4.166	14.06	19.6	4.346	9.31	9.8	4.646	4.65
39.4	4.042	18.71	29.4	4.169	13.96	19.4	4.350	9.21	9.6	4.655	4.56
39.2	4.045	18.62	29.2	4.172	13.87	19.2	4.354	9.12	9.4	4.665	4.46
39.0	4.047	18.52	29.0	4.175	13.77	19.0	4.359	9.02	9.2	4.674	4.37
38.8	4.049	18.43	28.8	4.178	13.68	18.8	4.364	8.93	9.0	4.683	4.27
38.6	4.051	18.33	28.6	4.181	13.58	18.6	4.368	8.83	8.8	4.693	4.18
38.4	4.053	18.24	28.4	4.184	13.49	18.4	4.373	8.74	8.6	4.703	4.08
38.2	4.056	18.14	28.2	4.187	13.39	18.2	4.378	8.64	8.4	4.713	3.99
38.0	4.058	18.05	28.0	4.190	13.30	18.0	4.383	8.55	8.2	4.724	3.89
37.8	4.060	17.95	27.8	4.194	13.20	17.8	4.387	8.45	8.0	4.735	3.80
37.6	4.063	17.86	27.6	4.197	13.11	17.6	4.392	8.36	7.8	4.746	3.70
37.4	4.065	17.76	27.4	4.200	13.01	17.4	4.397	8.26	7.6	4.757	3.61
37.2	4.067	17.67	27.2	4.203	12.92	17.2	4.402	8.17	7.4	4.768	3.51
37.0	4.070	17.57	27.0	4.206	12.82	17.0	4.407	8.07	7.2	4.780	3.42
36.8	4.072	17.48	26.8	4.210	12.73	16.8	4.412	7.98	7.0	4.793	3.32
36.6	4.074	17.38	26.6	4.213	12.63	16.6	4.418	7.88	6.8	4.805	3.23
36.4	4.077	17.29	26.4	4.216	12.54	16.4	4.423	7.79	6.6	4.818	3.13
36.2	4.079	17.19	26.2	4.219	12.44	16.2	4.428	7.69	6.4	4.831	3.04
36.0	4.081	17.10	26.0	4.223	12.35	16.0	4.434	7.60	6.2	4.845	2.94
35.8	4.084	17.00	25.8	4.226	12.25	15.8	4.439	7.50	6.0	4.860	2.85
35.6	4.086	16.91	25.6	4.230	12.16	15.6	4.445	7.41	5.8	4.874	2.75
35.4	4.089	16.81	25.4	4.233	12.06	15.4	4.450	7.31	5.6	4.889	2.66
35.2	4.091	16.72	25.2	4.236	11.97	15.2	4.456	7.22	5.4	4.905	2.56
35.0	4.094	16.62	25.0	4.240	11.87	15.0	4.462	7.12	5.2	4.921	2.47
34.8	4.096	16.53	24.8	4.243	11.78	14.8	4.467	7.03	5.0	4.938	2.37
34.6	4.099	16.43	24.6	4.247	11.68	14.6	4.473	6.93	4.8	4.956	2.28
34.4	4.101	16.34	24.4	4.250	11.59	14.4	4.479	6.84	4.6	4.975	2.18
34.2	4.104	16.24	24.2	4.254	11.49	14.2	4.485	6.74	4.4	4.994	2.09
34.0	4.106	16.15	24.0	4.258	11.40	14.0	4.492	6.65	4.2	5.015	1.99
33.8	4.109	16.05	23.8	4.261	11.30	13.8	4.498	6.55	4.0	5.036	1.90
33.6	4.111	15.96	23.6	4.265	11.21	....	.....	.....	....	.....	.....

but $\overline{A'E} = D\phi \sin \alpha_1 + D\lambda \cos \alpha_1$. Therefore,

$$d\alpha = - \frac{1}{s \sin 1''} (D\phi \sin \alpha_1 + D\lambda \cos \alpha_1)$$

or,

$$d\alpha = - K (D\phi \sin \alpha_1 + D\lambda \cos \alpha_1) \ldots\ldots\ldots\ldots\ldots (2)$$

in which $K = \frac{1}{s \sin 1''}$. It is readily apparent that practically the same rotational change occurs at both ends of the line, and $d\alpha$ must be applied to α_1 as well as to α_2.

Line AE, Fig. 3, represents, in meters, the change in the length of the line, AB. If this is multiplied by a factor, C, which is the tabular difference (in units of the last place) in logarithms per meter of s, then the change in log $s = d$ (log s) $= - C \times \overline{AE}$; but $\overline{AE} = D\lambda \sin \alpha_1 - D\phi \cos \alpha_1$, and therefore:

$$d (\log s) = C (D\phi \cos \alpha_1 - D\lambda \sin \alpha_1) \ldots\ldots\ldots\ldots\ldots (3)$$

For 6-place logarithms, $C = \frac{10^6 \log e}{s}$. Values of Factors C and K may be read from Table 2.

When the total latitude correction to both ends of the line is appreciable (as is the case between some of the old astronomic datums in the West and the 1927 North American Datum), and the difference in longitude between the ends of the line is large, another term must be added to the length correction to allow for convergence of meridians. This need becomes apparent when one considers that the distance between meridians decreases rapidly with an increase in latitude. For instance, if two points at the same latitude, but on different meridians, were shifted north by the same amount, the differential change between them would be zero in latitude and zero in longitude; but since the movements of the points were not parallel, the distance between them would be decreased appreciably, whereas the azimuth would remain practically unchanged.

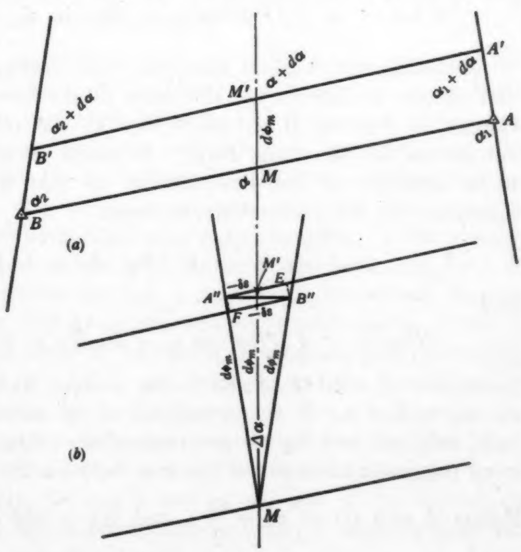


FIG. 4

To determine the change in length due to convergence, consider Fig. 4(*a*). Points A and B are moved northward along different meridians to A' and B', respectively. If M is the mid-point of the line, $\overline{AB}$, $\overline{MM'} = d\phi_m$, or the mean change in latitude of Stations A and B. In meters, $\overline{MM'} = Q \times d\phi_m$.

The next step will be clearer if the line is assumed to be broken at M, and the two halves are moved separately as shown in Fig. 4(*b*), so that MB'' is parallel to BB' and equal to $d\phi_m$, and MA'' is parallel to AA' and equal to $d\phi_m$. For the small angles involved, the arc, $A''\,M'B''$, may be treated as a straight line. Then, $\overline{B'B''} = \overline{BM}$; $\overline{A'A''} = \overline{AM}$; and, $\overline{AB} = \overline{A'A''} + \overline{B'B''}$. The shortening of the original line would then be the distance by which the two halves overlap, or, $-\ \delta s = \overline{A''E} = \overline{B''F} = A''B'' \sin{(\alpha + d\alpha)}$; but $\overline{A''B''}$ (in meters) $= Q\ d\phi_m\ \Delta\alpha$ arc $1'$. Since $d\alpha$ will usually be very small compared to α, $\sin \alpha$ may be substituted for $\sin{(\alpha + d\alpha)}$ with negligible error. Therefore,

$$-\ \delta s = Q \text{ arc } 1'\ d\phi_m\ \Delta\alpha \sin \alpha \ldots\ldots\ldots\ldots\ldots\ldots (4)$$

By substituting Q arc $1' = 0.009$, approximately, and noting that $\Delta\alpha \sin \alpha$ will always be negative, the correction becomes,

$$\delta s = 0.009\ d\phi_m\ \Delta\alpha \sin \alpha \ldots\ldots\ldots\ldots\ldots\ldots (5)$$

Since δs has been derived in meters, it should be applied to the change in length of s (Equation (3)) to give:

$$d(\log s) = C\ (D\phi \cos \alpha_1 - D\lambda \sin \alpha_1 + 0.009\ d\phi_m\ \Delta\alpha \sin \alpha) \ldots (6)$$

Although the original formulas were derived on the basis of a differential change at Station A, the same might have been done, instead, for the change at Station B, or preferably at the mid-point, M, and the results determined in the same form. It seems reasonable, therefore, to use the mean azimuth of the line instead of that at either end, and the final formulas for the corrections become:

$$d\alpha = -\ K\ (D\phi \sin \alpha + D\lambda \cos \alpha) \ldots\ldots\ldots\ldots\ldots (7)$$

and,

$$d(\log s) = C\ (D\phi \cos \alpha - D\lambda \sin \alpha + 0.009\ d\phi_m\ \Delta\alpha \sin \alpha) \ldots (8)$$

in which $D\phi = 30.83\ \delta\phi$; $\delta\phi = d\phi_1 - d\phi_2$; $D\lambda = R\ \delta\lambda$; $\delta\lambda = d\lambda_1 - d\lambda_2$; $d\phi$, $d\lambda$, $d\alpha$, and $d(\log s)$ = corrections to old datum values for latitude, longitude, azimuth, and log meters, respectively; $d\phi_m$ = the mean of $d\phi_1$ and $d\phi_2$; α = the mean azimuth of the line between Points (1) to (2), (or between Points A and B) $= \alpha_1 + \dfrac{\alpha\Delta}{2}$; and, $\Delta\alpha$ = the convergence of meridians, in minutes of arc, $= \alpha_2 - \alpha_1 \pm 180°$.

To facilitate the computations, the form in Fig. 5 has been prepared. It is arranged to hold the computations for four geodetic lines (an example is

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$$d\alpha = -K\,(D\phi \sin\alpha + D\lambda \cos\alpha)$$

$$d(\log s) = C\,(D\phi \cos\alpha - D\lambda \sin\alpha + 0.009\, d\phi_m\, \Delta\alpha \sin\alpha^*)$$

* The product $\Delta\alpha \sin\alpha$ will always be negative. Signs must be carried through algebraically

1. State *Arizona*		No. ----	State		No.
2. $d\phi$ *+5.17*	(1) *Alax*	$d\lambda$ *−4.24*	$d\phi$	(1)	$d\lambda$
3 $d\phi$ *+5.11*	(2) *Stonewhet*	$d\lambda$ *−4.35*	$d\phi$	(2)	$d\lambda$
4. $\delta\phi$ *+0.06*	Line (1) minus Line (2)	$\delta\lambda$ *+0.11*	$\delta\phi$	Line (1) minus Line (2)	$\delta\lambda$
5. $D\phi$ *+1.85*	Meters	$D\lambda$ *+2.89*	$D\phi$	Meters	$D\lambda$
6. $\sin\alpha$ *+0.914*	$\cos\alpha$ *−0.407*		$\sin\alpha$	$\cos\alpha$	
7. $D\phi \sin\alpha$ *+1.69*	$D\phi \cos\alpha$ *−0.75*		$D\phi \sin\alpha$	$D\phi \cos\alpha$	
8. $D\lambda \cos\alpha$ *−1.18*	$-D\lambda \sin\alpha$ *−2.64*		$D\lambda \cos\alpha$	$-D\lambda \sin\alpha$	
9. Sum *+0.51*	$0.009\, d\phi_m$ $\Delta\alpha \sin\alpha^*$ *−0.42*		Sum	$0.009\, d\phi_m$ $\Delta\alpha \sin\alpha^*$	
10. $d\alpha$ (sec) = $-K \times$ Sum *−3.4*	Sum *−3.81*		$d\alpha$ (sec) = $-K \times$ Sum	Sum	
11. ϕ *32* Log s *4.494*	$d(\log s)$ = $C \times$ Sum *−53.1*		ϕ Log s	$d(\log s)$ = $C \times$ Sum	
12. α *114* $\Delta\alpha$ *10*	By ----	Date ----	α $\Delta\alpha$	By	Date

FIG. 5.—SAMPLE FORM FOR COMPUTATIONS. (COMPLETE FORM PRINTS COMPUTATIONS FOR FOUR GEODETIC LINES).

shown in Fig. 5), which may be performed conveniently in the following order, using a slide-rule for all multiplications:

The State in which the line occurs and the reference number of the computation are entered on Line 1 of the form; the names of the stations involved, together with their respective latitude and longitude corrections, on Lines 2 and 3; the mean latitude and azimuth, log distance, and convergence, on Lines 11 and 12, as indicated. All these values may be taken by inspection from the available records for the two stations involved. For third-order triangulation, with positions given to the nearest 0.01″ of latitude and longitude, azimuth to the nearest second, and log s to six decimal places, it is sufficient to use ϕ and α to the nearest degree, log s to three decimal places, and $\Delta\alpha$ to the nearest minute on the form.

Next, $\delta\phi$ and $\delta\lambda$ are determined by algebraic subtraction and entered on Line 4, and $D\phi$ and $D\lambda$ are computed and entered on Line 5. Note that R is taken from Table 1, using ϕ as the argument; sin α and cos α are taken from tables, using α as the argument, and are entered on Line 6.

With $D\phi$ set on the slide-rule, $D\phi$ sin α and $D\phi$ cos α are determined and entered on Line 7. Similarly, $D\lambda$ cos α and $-$ $D\lambda$ sin α are entered on Line 8; $d\phi_m$ is determined by inspection from Lines 2 and 3; and the term, $0.009\ d\phi_m\ \Delta\alpha \sin\alpha$ is entered on Line 9.

The sums on Lines 9 and 10, Fig. 5, are obtained algebraically as indicated; Factors C and K are taken from Table 2, using log s as the argument, and applied to the sums as indicated to obtain $d\alpha$ and $d(\log s)$ on Lines 10 and 11, respectively. The initials of the computer and the date are entered on Line 12.

Since C and K are direct functions of $\frac{1}{s}$, to find either, for a line for which the value of log s is beyond the limits of Table 2, find the value desired for the log s in the table with the same mantissa, and multiply or divide by the proper power of 10; for instance, C and K for $\log s = 2.200$ are 100 times C and K for $\log s = 4.200$, respectively; and for $\log s = 5.200$, the values are 0.1 times those for $\log s = 4.200$, respectively. In other words:

When log s has a characteristic of:	Multiply Factors C and K, Table 2, by:
2	100
3	10
5	0.1

Table 2 gives C for 6-place logarithms. For additional place, multiply the tabular value by 10; for each place less than 6, divide by 10. Table 2 is arranged for intervals of 0.2 for C and 0.095 for K.

With all the necessary information before him, the average computer should be able to determine the corrections for a geodetic line in from 3 to 5 min. The work can be speeded considerably by setting up a number of geodetic lines on the forms and computing them together, completing each operation on the entire group before proceeding with the next. If a colored pencil is used for recording the last step in the computations, namely, $d\alpha$ and $d(\log s)$, these values will stand forth conspicuously, and may be found more readily when needed.

Nomographic Chart

In order to check the values of $d\alpha$ and $d(\log s)$ computed by the formulas, the nomographic chart of Fig. 6 has been prepared, on which the same corrections may be determined graphically. It has scales for $\delta\phi$ (the differential change in latitude within the line); $\delta\lambda$ (the differential change in longitude at the various latitudes); sin α; cos α; log s; and the desired corrections, $d\alpha$ and $d(\log s)'$. In addition, there is an auxiliary chart for the determination of the supplementary correction to $d(\log s)'$, due to the convergence of meridians.

To use Fig. 6, proceed as follows: To determine $d\alpha$, mark on the proper scales, $\delta\phi$ (Point a), $\delta\lambda$ (Point b) for the proper latitude, sin α (Point c), cos α (Point d), and log s (Point e). On the $\delta\lambda$-scale, project the intersection of the horizontal line for ϕ and the curved line for $\delta\lambda$ to the main horizontal line, (Point g), using as a guide the fine vertical lines.

Connect log s with sin α (Line ce) and draw a parallel line (ah) through $\delta\phi$ to intersect the vertical index line containing the sin and cos scales. Call this intersection Point (h). Connect log s with cos α (Line de) and draw a parallel line through $\delta\lambda$ (Line gk) to intersect the vertical index line on the right half of the chart. Call this intersection Point (k). The line joining Points (h) and (k) intersects the $d\alpha$-scale *at* Point l, indicating the value of $d\alpha$.

To determine d(log.s), the markings for $\delta\lambda$ (Point g), sin α (Point d), and cos α (Point c) are the same as before: $\delta\phi$ (Point a) must be set off on opposite side of the $\delta\phi$-scale, and log s (Point e) must be marked on the lower log s-scale.

Connect log s and cos α (Line ec) and draw a parallel line (ah) through $\delta\phi$, intersecting the left index line as before. Call this intersection Point (h). Connect log s and sin α (Line de), and draw a parallel line (gk) through $\delta\lambda$, intersecting the right index line as before. Call this intersection Point (k). The line joining Points (h) and (k) intersects the d(log s)$'$-scale at the preliminary value for d(log s) Point (l).

Fig. 7, an auxiliary chart for the convergence term, has scales for log s, α, $\Delta\alpha$, $d\phi_m$ (the mean latitude correction), and d(log s)$''$. To use this chart, draw a line (ab) connecting log s with $\Delta\alpha$. Through the intersection of this line with the diagonal, draw a line from $d\phi_m$ (Point c), to intersect the right vertical index (Point d). Through this intersection and α (Point e) on the diagonal scale, draw a line (def) to meet the left scale at d(log s)$''$, which will always be opposite in sign to $d\phi_m$. This value for d(log s)$''$ should be added to the preliminary value, d(log s)$'$, obtained from Fig. 6, to derive the final value of d(log s).

For use in the office, the nomographic chart has been drawn on a double-mounted plane-table sheet, 18 by 24 in. in size, and covered with a sheet of transparent cellulose acetate. All marks and lines for each problem are drawn in pencil on this cover, from which they may be easily removed later with a soft eraser, leaving the chart clean for the beginning of another problem. Parallel lines are drawn by the usual methods, using a parallel rule or a pair of triangles.

A complete problem (determination of $d\alpha$ and d(log s)) can be solved in about 2 min by one familiar with the charts.

Two-Point "Isodiffs"

Examination of the Wilson method indicates that two tie-points are sufficient to control the adjustment of a triangular net if no others are available. Since this is true, it should be possible to draw "isodiffs" to give, graphically, the same adjustment that would be obtained by the application of the Wilson formulas. Interpolation along the line between the two tie-points, however, gives only the spacing of the "isodiffs", but no indication of their directions. It should be possible to compute, by the formulas, the position changes of one additional point on the system, and

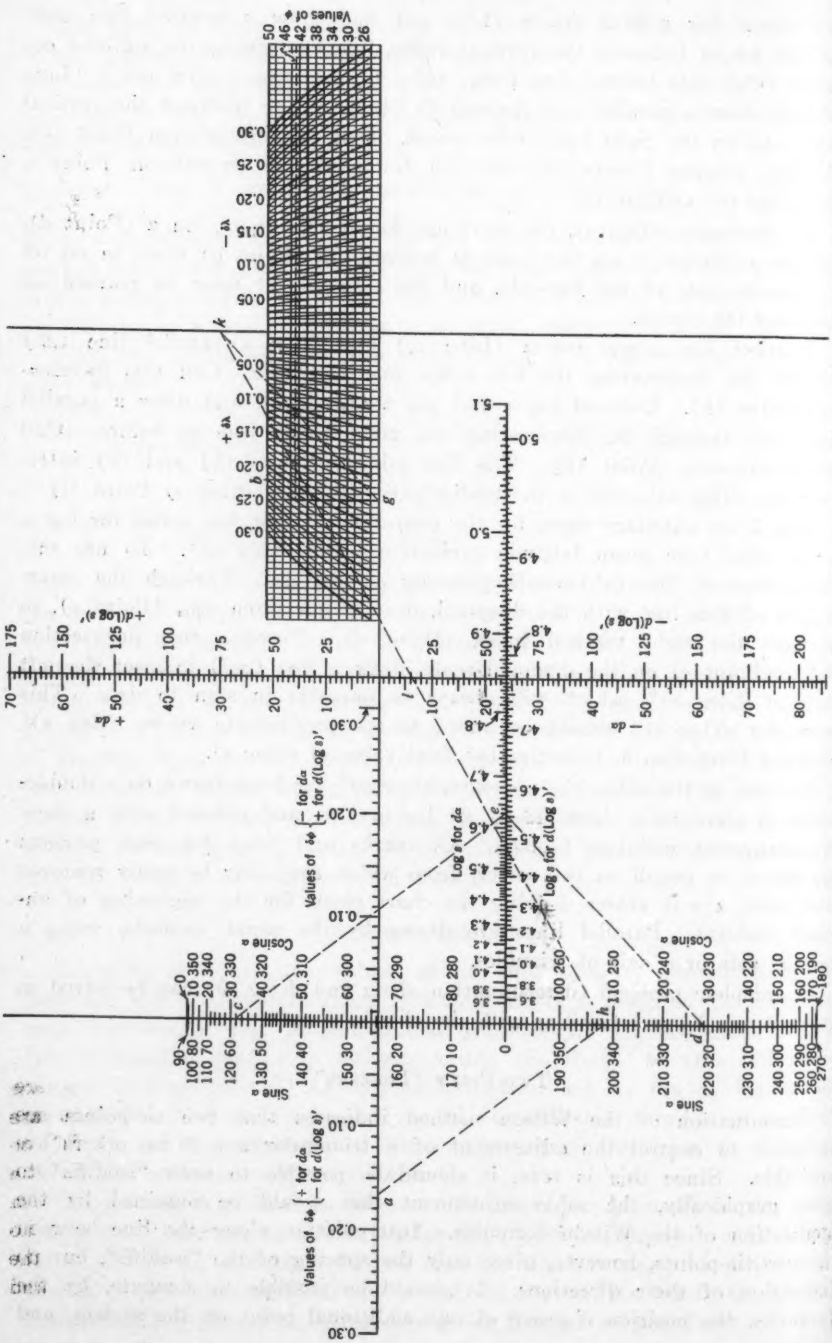

FIG. 6.—NOMOGRAPHIC CHART FOR THE DETERMINATION OF THE SWING AND CHANGE IN LENGTH OF A LINE RESULTING FROM THE SHIFTING OF ITS ENDS.

to use that as the third control point for the "isodiffs". This would replace the analytical method for computing position changes by a speedier graphical method.

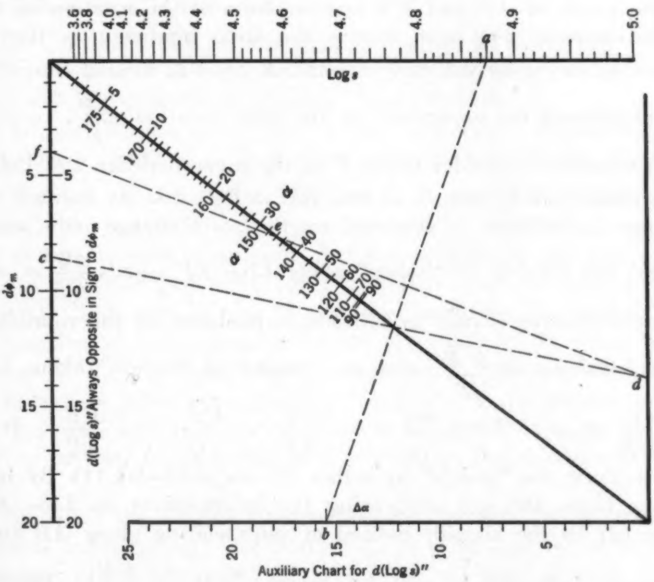

Fig. 7.

The computation of the third control point, requiring, as it does, the determination of the various constants, could be dispensed with if a means were found to draw the "isodiffs" from only two points as a base. This means can be found by applying graphically the principles of the Wilson method.

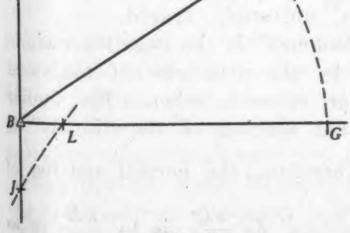

Fig. 8.

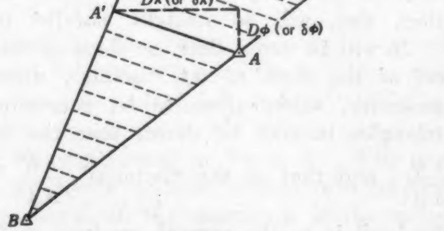

Fig. 9.

Assume, for instance, that Ties A and B (Fig. 8), are available with corrections of $d\phi_1$, $d\lambda_1$ and $d\phi_2$ $d\lambda_2$, respectively. As before, determine

$\delta\phi$, $\delta\lambda$, $D\phi$, and $D\lambda$. If $D\phi$ and $D\lambda$ are plotted at A to an enlarged scale as before, Point A' becomes the new relative position of Station A, and Point E the projection of Point A' on Line AB. Then, AE represents the change in length of AB and $A'E$ is a measure of the rotation of the base. Since the entire system must receive the same treatment as the base, all lines must be swung by the same amount, $K \times \overline{A'E}$, around Station B as a pivot, and reduced (or expanded) by the same rotation, $\frac{\overline{AE}}{s}$.

For convenience, consider Point F on the same meridian and Point G on the same parallel as Station B, so that $\overline{BF} = \overline{BG} = s$. At Point F the relative change in latitude is produced by the scale change only and equals $\frac{-\overline{AE}}{Q}$, and the spacing of "isolats" along Line BF equals $\frac{sQ}{\overline{A\,E}} = m_\phi$. At Point G the relative change in latitude is produced by the rotation factor, $A'E$, only, and equals $\frac{\overline{A'E}}{Q}$, and the spacing of "isolats" along Line BG equals $\frac{s\,Q}{\overline{A'E}} = p_\phi$. Then, $\frac{m_\phi}{p_\phi} = -\frac{s\,Q}{\overline{AE}} \times \frac{\overline{A'E}}{s\,Q} = -\frac{\overline{A'E}}{\overline{AE}}$. It is now possible to draw the "isolats" by either of two methods; (1) By interpolation along Lines BF and BG, using the intersections on Line AB as a check; or (2) by the simpler method of interpolating along AB and using the diıections, indicated by the slope ratio, $\frac{m_\phi}{p_\phi} = -\frac{\overline{A'E}}{\overline{AE}}$. The "isolats" will be straight, parallel lines, uniformly spaced.

Similarly, the relative longitude change at Point F is produced by $A'E$ only, and equals $\frac{\overline{A'E}}{R}$, and the spacing of the "isolongs" along BF equals $\frac{s\,R}{\overline{A'E}} = m_\lambda$; also, the relative longitude change at Point G is produced by AE only, and equals $\frac{\overline{AE}}{R}$, and the spacing of the "isolongs" along Line BG equals $\frac{s\,R}{\overline{AE}} = p_\lambda$. Then, $\frac{m_\lambda}{p_\lambda} = \frac{s\,R}{\overline{A'\,E}} \times \frac{\overline{AE}}{s\,R} = \frac{\overline{AE}}{\overline{A'\,E}}$. The "isolongs" may be drawn by the same methods as those used for the "isolats", and they, also, will be straight, parallel lines, uniformly spaced.

It will be noted that the slope of the "isolongs" is the negative reciprocal of the slope of the "isolats", which, by the principles of analytical geometry, shows them to be perpendicular to each other. By similar triangles it may be shown that the normal spacing of the "isolats" is $\frac{s\,Q}{\overline{A\,A'}}$, and that of the "isolongs", $\frac{s\,R}{\overline{AA'}}$. Therefore, the normal spacing of "isolats" is to the normal spacings of "isolongs" as $\frac{s\,Q}{\overline{AA'}}$ is to $\frac{s\,R}{\overline{AA'}}$ or as Q is to R.

Special attention may now be called to certain fundamental features of the "isodiff" diagram. Whenever a net of triangulation is shifted so that no change occurs in any of the previously adjusted angles of the net, the following conditions must result:

(*a*) The "isolats" will be straight, parallel lines, uniformly spaced;

(*b*) The "isolongs" also will be straight, parallel lines, uniformly spaced, and they will be perpendicular to the "isolats"; and,

(*c*) The normal spacing of the "isolats" will be to the normal spacing of the "isolongs" as Q is to R for the mean latitude of the project.

Conversely, it would appear that, when a graph of "isodiffs" satisfies Conditions (*a*), (*b*), and (*c*), the shape of the triangulation net being adjusted is not changed or warped. In other words, the net may have been shifted, rotated, and expanded (or contracted), but the angles have not been altered. This will be true if one other condition is satisfied; namely, if the direction of increasing values of the "isolats" is taken as north, the values of the "isolongs" should increase to the westward. If this pattern is kept in mind when drawing "isodiffs" for the general problem of several tie-points, an effort can be made to adhere to it as far as possible, in order to prevent or to minimize distortion in the system that is being adjusted.

To summarize the simpler construction for "isodiffs" when only two ties are available for a system: At Station A, plot $D\phi$ and $D\lambda$ to locate Point A'. Measure Lines AE and $A'E$. Along the meridian through Station B (BF), lay off $BJ = - A'E$. Along the parallel through Station $B(BG)$, lay off $BL = AE$. Interpolating along AB for the "isolat" intersections, draw "isolats" parallel to JL. Interpolating along AB for the "isolong" intersections, draw "isolongs" perpendicular to JL. These "isodiffs" may be extended beyond Points A and B, respectively, by continuing the same spacings. As a partial check on the work, it may be noted that the normal distance between "isolats" is to the normal distance between "isolongs" as Q is to R.

From these "isodiffs" the same position corrections should be obtained as by the Wilson formulas; and the uniform azimuth and distance corrections may be applied to all lines in the same manner.

ROTATING GUIDE

Another method has been developed by Mr. Nelson for the graphical application of the principles proposed by Mr. Wilson. On a stiff transparent medium, such as celluloid or cellulose acetate, lay off Line (AB) Fig. 9). At Station A, plot $D\phi$ and $D\lambda$ to locate Point A'. Then, AA' represents the actual movement (to the distorted scale) of Point A. With the system pivoted about Station B, all points on the system will receive the same relative movement with respect to B, the magnitude of the movements varying only with the distance of each point from the pivot. If BA and BA' are extended indefinitely, any point the original position of

which is on BA would fall on BA' after adjustment, and the path of its movement would be parallel to AA'.

To use the adjustment guide, pass a pin or thumb-tack through Point B on the guide and Point B on the map, so that the guide is free to pivot about that point. Rotate the guide about the pivot so that each station to be adjusted will, in turn, fall on BA, extended. Project the station to BA' by a line parallel to AA'. Measure the differences in latitude and longitude for each station to the scale of the guide, and divide by Q and R, respectively, to obtain $(d\phi)'$ and $(d\lambda)'$. The projection to BA' may be made by eye if a number of lines are ruled on the guide parallel to AA'.

When a number of stations are to be adjusted by this method, time can be saved if scales for $(d\phi)'$ and $(d\lambda)'$ for the mean latitude of the project, are prepared in which one unit of $(d\phi)'$ is to one unit of $(d\lambda)'$ as Q is to R.

With the aid of these auxiliary scales, $\delta\phi$ and $\delta\lambda$, for Point A, may be plotted directly on the guide without first converting them to meters, and $(d\phi)'$ and $(d\lambda)'$ may be scaled in seconds instead of in meters. Of course, the corrections at Station B must be added to those obtained by the use of the guide, in order to secure the final corrections for each station being adjusted. The uniform aximuth and distance corrections may be applied to each line in the proper manner.

Conclusion

It is readily apparent that all the methods of adjustments herein described are based on approximations of varying degrees. However, since the actual quantities considered are so small, they may be determined only approximately and still be well within the accuracy of the original survey. The mathematical computations will seldom require more than three significant figures, and a 10-in. slide-rule will usually be found ample; for the graphical work, ordinary plotting precision will satisfy most requirements.

The weakest link in the entire chain of readjustment would appear to be the construction of the "isodiffs" when several ties are available. Moreover, these methods do not correct errors or inaccuracies that may exist in the original computations. In the readjustment of good field work that has been rigidly adjusted to a good datum, the change of datum will not be likely to distort the originally adjusted figure to any great extent, so that the "isodiffs" should be sketched with little difficulty and will be fairly regular lines, well spaced. For such work there should be little question about the suitability of readjustment by "isodiffs".

If the field work itself contains appreciable discrepancies, or if it was not properly adjusted, or adjusted to a poor datum, the corrections at the tie-points might not be expected to be consistent. The "isodiffs" for such a readjustment might be badly curved and poorly spaced, but the readjustment based on them would probably be no worse than the original work and would make the survey available on the new datum, whereas its usefulness might otherwise be lost completely.

These methods, furthermore, permit the closing of circuits that formerly consisted of independent links tying to common stations, but with different co-ordinates. In some problems of this type, it is a simple matter to sketch the 'isodiffs" between the fixed ties, making due allowance for the break.

Another method that sometimes gives good results is to assume as a position for each common station a weighted mean of its positions on the respective triangulation nets tying to it, the weight depending on the strength of the respective nets and the distance of the common station from each of the fixed tie-points. The common station may then be used as a fixed tie-point in the adjustment.

A third method, somewhat longer than either of the foregoing but, perhaps, more rigid, involves a partial preliminary adjustment. Each link in the circuit is, in turn, adjusted to the datum of any one of the links, regardless of the fixed tie-points. When the entire circuit has been reduced to a single unified datum it may be adjusted to the final datum through the fixed tie-points.

Other appropriate methods will doubtless suggest themselves to the computer, according to the specific problems on hand.

The use of these methods for readjustment of old work suggests an application to new work that might warrant the consideration of the computer. If a chain of quadrilaterals (or other suitable figures) is extended from one fixed base to close on another, it should be possible to begin with the fixed positions at one end of the chain and compute through individually adjusted figures to obtain new values for the fixed positions at the other end. The necessary corrections to bring the closing stations to their proper positions may then be determined, and "isodiffs" drawn for the chain, using the corrections determined for the closing end and zero corrections for the other end. Then, all intermediate positions could be corrected by the indicated amounts, and all lines corrected for azimuth and scale by the formulas. By this means, latitude and longitude conditions, as well as length and azimuth conditions, would be satisfied for both bases. Although the results might not agree perfectly with those of a complete, rigid least-squares adjustment, it is believed the differences would be very slight unless the strength of the chain varies appreciably within its length.

The choice of methods for readjustment of datum may be summarized briefly, as follows:

(*a*) When two ties to the new datum are available, determine position corrections by: (1) The Wilson formulas; (2) the rotating adjustment guide; or (3) the two-point "isodiffs". Apply uniform azimuth and length corrections to all lines.

(*b*) When three or more ties are available, construct an "isodiff" graph for the determination of position changes. Determine azimuth and length corrections by the formulas or by the nomographic chart; or use both for a check.

AMERICAN SOCIETY OF CIVIL ENGINEERS
Founded November 5, 1852
DISCUSSIONS

STABLE CHANNELS IN ERODIBLE MATERIAL

Discussion

By E. W. Lane, M. Am. Soc. C. E.

E. W. Lane,[47] M. Am. Soc. C. E. (by letter).[47a]—The constructive nature of the discussion of this paper is gratifying, and the writer feels that its value has been materially increased thereby. Although these discussions have shown considerable differences of opinion on minor phases, there seems to be little criticism of the major contentions of the paper, and no points were subject to a general attack. The importance of silt quantity as a factor in stable channel shape and the tendency of heavily silt-laden water to form wide, shallow channels seems to have been generally accepted. Since the items of criticism or questions raised by those discussing the paper cover a wide range of ideas and in few cases is the same question raised by more than one person, this closure can best be presented by dealing with the various discussions individually.

The empirical relations derived by Mr. Lacey seem to have been widely accepted in India. The writer believes that they are probably the best quantitative relations yet developed. The striking agreement with the results obtained by Colonel Pettis on an entirely independent basis indicates strongly the general accuracy of the conclusions of both men for average conditions. It is believed, however, that at least one important factor has been omitted, namely, the concentration of the silt load, and when the load is high, this becomes such an important factor that it must be considered.

Mr. Lacey suggests a plotting of the values, $\frac{P}{R}$, against V for the Imperial Valley canals, which, to agree with his fundamental shape formula, should show $\frac{P}{R} = 7.12\,V$. This has been done in Fig. 13.

Note.—The paper by E. W. Lane, M. Am. Soc. C. E., was published in November, 1935, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: February, 1936, by R. C. Johnson, M. Am. Soc. C. E.; April, 1936, by Messrs. E. S. Lindley, J. C. Stevens, C. R. Pettis, Harry F. Blaney, and Sigurd Eliassen; May, 1936, by Messrs. R. E. Ballester, and Gerald Lacey; and August, 1936, by Messrs. V. V. Tchikoff, and W. M. Griffith.

[47] Prof., Hydr. Eng., Univ. of Iowa, Iowa City, Iowa.

[47a] Received by the Secretary April 13, 1937.

The solid black dots representing the results of sections of the canals reported by William T. Collings, Jr., M. Am. Soc. C. E., as stable[48] and the small open circles give the dimensions of ditches cited by Mr. Blaney,

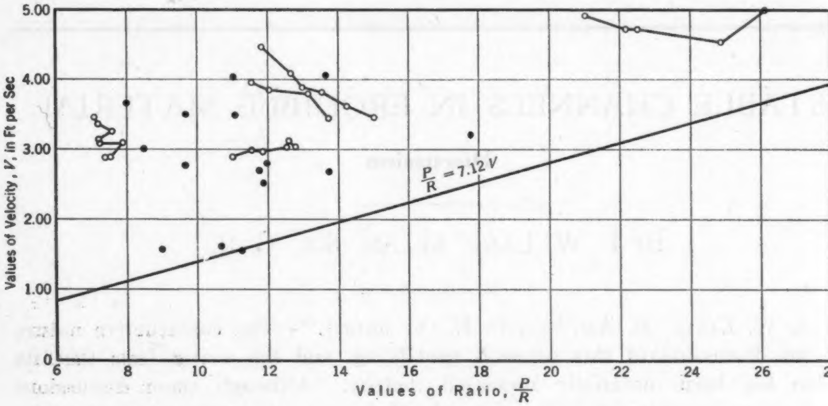

FIG. 13

in Table 5, assuming that the sides are vertical. The values given scatter considerably, even for the same ditch and station, but with one exception, all of them show the ratio, $\frac{P}{R}$, to be less than 7.12 V. Although the accuracy of individual observations may be questioned, the data very strongly indicate that the Lacey shape formula does not fit Imperial Valley conditions. This lack of agreement is probably the result of the high silt concentration in these canals.

The writer does not believe that the factor, $\frac{V^2}{R}$, empirically derived by Mr. Lacey and also independently by Mr. Tchikoff, has been demonstrated to be a rational measure of turbulence, although it may be a very convenient empirical index of it. Some of the arguments advanced in support of it do not appear to be valid. For example, the fact that the ratio, $\frac{V^2}{R}$, is a valuable criterion in non-uniform flow seems to be pure coincidence and to bear no relation to the phenomena of stable channel shapes. Assuming that all the energy loss due to the friction of the water flowing in a ditch is dissipated by turbulence, the average turbulence of a stream (under conditions of steady, uniform flow), expressed in terms of the energy loss per unit time per unit of volume (occupied by a unit weight), is proportional to the product of the slope and the mean velocity of flow. This follows from the fact that if the water is not accelerating, the energy dissipated per unit of volume per unit of time must be equal to the work represented by the drop of the

[48] *Transactions*, Am. Soc. C. E., Vol. 99 (1934), p. 554.

water in that time, which, in turn, is equal to the mean velocity of the water times the slope. Hence, the criterion of turbulence should be, $S\,V$. Although some of the loss, no doubt, occurs in the laminar flow of the boundary layer on the sides and bottom of the ditch, it is believed that under ordinary conditions of an earth channel this is a small part of the total. If the Manning formula is assumed to evaluate, correctly, all the variable factors, this criterion can be transformed as follows:

$$S\,V = \frac{V^3}{\left(\frac{1.486}{n}\right)^2 R^{\frac{4}{3}}} = \left(\frac{n}{1.486}\right)^2 \left(\frac{V^2}{R^{\frac{8}{9}}}\right)^{\frac{3}{2}} \quad \ldots\ldots\ldots\ldots (57)$$

or, in other words, $SV \propto \left(\frac{V^2}{R^{\frac{8}{9}}}\right)^{\frac{3}{2}}$. If the Lacey formula,

$$S = \left(\frac{N_a}{1.3458}\right)^2 \frac{V^2}{R^{\frac{3}{2}}} \quad \ldots\ldots\ldots\ldots\ldots\ldots (58)$$

is used instead of that of Manning, the expression, $S\,V$, becomes $\left(\frac{N_a}{18.534}\right)^2 \left(\frac{V^2}{R}\right)^{\frac{3}{2}}$. Thus, it will be seen that using the Lacey formula, the empirical Lacey (and Tchikoff) criterion, $\frac{V^2}{R}$, is brought into almost perfect agreement with the criterion, $S\,V$. This probably explains why a criterion, $\frac{V^2}{R}$, gives such good results, especially when used with the Lacey formula instead of the Manning formula.

Mr. Lacey advocates the use of the Buckingham method for future silt studies. This method, unquestionably, is a useful tool, but can only be used safely if all the important dimensioned variables are known, and it will indicate nothing as to the effect of the dimensionless factors.

It is not believed that the conditions in the Imperial Valley canals approach the condition of flume traction, as suggested by Mr. Lacey. In nearly all cases, the material through which these canals flow is composed of the same kind of silt as is carried by the ditches, since the material was formed by the deposits of the same river, and therefore, the Imperial Valley canals are flowing in a self-borne alluvium in a manner similar to that of the canals of India. A good description of conditions in the Imperial Valley has been given by H. T. Cory, M. Am. Soc. C. E.[49] The writer's discussion of scour of the sub-grade when the flow in the channel is capable of carrying more material than enters at the head-gate, was included to cover the entire range of cases. It does not apply generally to the canals of the Imperial Valley.

The flow conditions in the Imperial canals are quite different from those found by Gilbert, and saltation is not believed to occur to an

[49] "Irrigation and River Control in the Colorado River Delta," *Transactions*, Am. Soc. C. E., Vol. LXXVI (1913), p. 1204.

important extent. Even the coarser particles of the Imperial Valley material are much smaller than the finest material used by him. Observations were made in the laboratory of the U. S. Bureau of Reclamation on the flow in a long flume with glass panels in the side, using bed material brought from the Colorado River. This showed that at ordinary velocities there was a very slight motion of the material along the bottom as bed load, forming ripples similar to those which appear on the bottom of the Imperial Valley canals, but nothing resembling saltation was observed, and the greater part of the material traveled in suspension. At very high velocities the ripples disappeared and the bottom became smooth, with great quantities of material moving in suspension. This smoothing out of the bottom in Colorado River material at high velocities was also observed by C. A. Wright, M. Am. Soc. C. E.

Mr. Stevens gives an interesting comparison of the sections of several rivers with the Lacey formula, Equation (5), and notes the lack of agreement for the values of the Colorado River at Yuma, Ariz., and the Yellow River, at Chiang-kou, China. The conditions of the Colorado River at the Yuma gaging-station are very unusual, and do not conform to the requirement of banks and bed of alluvium postulated by the Lacey relation. This may account for the lack of agreement. A short distance above the gaging section at Yuma, the Colorado River contracts very suddenly from a wide, meandering stream and passes through a narrow gorge about 400 ft wide, with sides of very resistant material of clay and boulders. At the gaging section, a short distance down stream from this throat, it is about 500 ft wide, and also has very permanent banks, possibly formed of the sandstone rock which outcrops only a short distance away. To pass through the narrow gap during periods of high discharge the water of the river must have a high velocity, which scours the light bed material to a great depth. When the discharge falls, the channel refills. The writer does not believe that this is typical of the Colorado River as a whole, as soundings in an unconfined section made elsewhere show no change of the mean bed elevation with high or low water. Moreover, for such scour to occur along the entire river during high water would require the transportation by the river of much larger quantities of silt than are shown by any measurement. The conditions at the Yuma gaging station, therefore, are a very special case, and conclusions based upon what happens there, if generally applied, will be very misleading.

The measurements on the Yellow River given by Mr. Stevens seem to be those recorded by the late John R. Freeman, Past-President and Hon. M. Am. Soc. C. E.[50] At this section there was a rock ledge on the right bank and rock also appeared on the left bank some distance back from the river. It is doubtful, therefore, whether this station is in the class for which Mr. Lacey claims his relations to hold.

Mr. Stevens is probably correct in his opinion that it is the momentary "stray currents" which are most effective in moving bed material, and the same probably applies also to the movement of the material at the sides of

[50] *Transactions*, Am. Soc. C. E., Vol. LXXXV (1922), p. 1437, Fig. 16.

the ditch. The writer does not believe that it is the difference in the velocity of the adjacent filaments of water which in itself causes the movement of the material, but that in those parts of the cross-section of a ditch where the isovels are close together the velocity differences within a small distance are great and, therefore, the turbulence is large. The spacing of the isovels, therefore, is an indication of turbulence, close spacing indicating high, and wide spacing indicating low, turbulence. Although this relation is believed to be true for the ordinary conditions of ditch flow where the turbulence is introduced by friction, it does not necessarily hold where the turbulence is the result, for example, of a hydraulic jump.

The data on the silt-carrying ability of the Wei Pei Main Canal which Mr. Eliassen presents, and that of Mr. Ballester for the Rio Negro Canals, are very valuable. Very little complete data of that kind are available and until it is collected, the carrying capacity of channels for solids will be only imperfectly known.

Mr. Eliassen's description of the heavy silt loads of the Chinese rivers is interesting. Unquestionably, he is correct in his remarks that a silt percentage should be a ratio of silt weight to the weight of silt plus water. In most conditions the silt concentrations are so small that to omit the silt weight from the silt-plus-water weight makes a negligible difference, but with the concentrations mentioned by Mr. Eliassen such a procedure leads to important errors.

The writer is not inclined to share with Mr. Eliassen the belief that bed load is an important factor in streams that carry a heavy suspended silt load. Measurements were made in the Colorado River on the crest of Laguna Dam where many years ago the river had filled the storage space with silt to the crest level, and, therefore, where the load measured was the combined bed and suspended loads. At the same time, measurements were made in a section five miles up stream, where only the suspended load was sampled. A comparison of the results over a considerable period of time showed very little difference,[51] indicating a relatively small bed load. That this is the case is confirmed by the observations in the flume previously mentioned. No bed-load measurements have been made for material comparable in size with that of the Colorado River, but the formulas derived for coarser material were extrapolated by Samuel Shulits, Assoc. M. Am. Soc. C. E., to the size of the Colorado River material, and the loads computed were found to be large, but in comparison with the very large suspended load, are relatively small. Still another reason for believing that the bed is not, to any great extent, in motion is that when sounded with a rod it is surprisingly hard, and does not have the much softer feeling which characterizes coarser material moving as bed load. As a result of these four independent lines of approach all of which are in agreement, the writer believes that in streams carrying high concentrations

[51] "Why Desilting Works for the All-American Canal," by C. P. Vetter, M. Am. Soc. C. E., *Engineering News-Record*, Vol 118, March 4, 1937, p. 322.

of fine material in suspension, the bed load, although perhaps of considerable absolute magnitude, is a small part of the total load.

The changes of roughness of the Yellow River channel mentioned by Mr. Eliassen may be due to the elimination of the ripples on the bottom observed at high velocities in the flume, as previously described. Such changes occur also in the Mississippi. In this case they are probably due to bed-load movement, as Mr. Eliassen suggested, as waves reaching more than 30 ft in height have been observed in the Mississippi River which, compared with the Colorado and Yellow Rivers carries a very small concentration of suspended material. For reasons previously stated, however, the writer does not believe that such waves form in rivers carrying heavy loads of suspended material.

Mr. Lindley objects to the writer's statement that he did not mention the quantity of silt as a factor in stable channel shape, explaining that the effect of the quantity of silt was so well known in India as to need no mention. There is no doubt that, in the literature from India, accessible to the writer when his paper was written, occasional mention was made of the fact that the quantity of silt was a factor, but in no place had he found any one who gave it the important place to which he believed it was entitled. The writer's emphasis of this phase was largely due to the fact that the Lacey relations, which were gaining wide acceptance in India, made no mention of the effect of the concentration of the silt load as a factor. Since preparing his paper the writer has secured a copy of the *Punjab Irrigation Technical Review* of 1925, and finds there a statement by Mr. Lindley (as well as others by Messrs. W. G. Quinton, A. R. Murray, and A. M. R. Montagu) that both quantity and quality are factors in determining the channel shape. Except for this publication (which is relatively inaccessible to American engineers) in no recent publication from India or elsewhere at the time this paper was written had the writer found the effect of silt concentration mentioned as an important factor.

Mr. Griffith states that it was well known that channels carrying heavy silt loads adopt broad shallow cross-sections. At this distance from India, it is possible for an American engineer to learn what was known there only from their written literature. From a very thorough study of extensive literature from India and other countries the writer found, as late as November, 1935, not a single mention of this phenomenon, and has found since only casual mention of it by Messrs. F. W. Woods and W. G. Quinton in the relatively inaccessible publication previously mentioned. Under these circumstances no apology for discussing it seems to be required.

Mr. Lindley suggests that the writer should have emphasized the fact that a given volume of flowing water carrying a given proportion of a given quality of silt tends to form a channel of which the depth, width, and gradient is all fixed by the said discharge and silt load. The writer's discussion suggested that a given discharge with a given quality and

quantity of silt did fix a definite cross-section of channel The gradient would follow from these by the ordinary laws of flow in open channels and, therefore, could not be considered an independent variable. Fixing the shape of the cross-section for a given discharge, therefore, fixes the gradient which would be stable, and it is not necessary to discuss it separately.

Not much benefit will result from arguments over the relative merits of the mean depth and the hydraulic radius as the hydraulic property to which the critical velocity is related. Mr. Griffith plots the available data on the basis of mean depth and finds it follows, reasonably well, an exponential relation to the critical velocity. Mr. Lacey uses the same data and finds an equally good agreement using the hydraulic radius. Both claim this as important evidence of the correctness of their formulas. The facts probably are that, within the range of channel conditions ordinarily encountered, the ratio of the mean depth to the hydraulic radius varies so little that the discrepancy is much less than that due to other factors.

The writer was seeking to obtain the form of cross-section which would be stable for the various conditions of discharge and silt characteristics, not necessarily the depth-width relation which would give the channel its greatest silt-carrying capacity for a given discharge and surface slope, as Mr. Griffith concluded. His determination of the depth of channel that would give the greatest silt-carrying capacity for a given condition may have value, but it seems to rest entirely upon his assertion that the capacity is a maximum when the ratio, $\frac{V}{dm^n}$, reaches its highest value, since no proof to support this assertion was submitted.

Mr. Tchikoff's proposition of "sediment equivalent" is a very ingenious one which merits thorough experimental investigation and testing by comparison with observed data in natural streams. It has the advantage of reducing the very complex relation of the quantity and quality of the silt present in a stream at a given time, to a single number. It is unfortunate that more quantitative data are not available to check this and the other interesting hypotheses suggested by Mr. Tchikoff. The collection of quantitative data seems to be one of the greatest needs in this field of science, for until hypotheses can be adequately checked against observed relations it is unsafe to build upon them.

Referring to Mr. Tchikoff's criticism of the position of the isovels in the cross-section given in the paper, the fact that the isovel, 0.80, is nearest the sides does not mean that the isovels of the lower values were non-existent but merely that they could not be plotted because of the lack of space. These velocity distribution charts were drawn from data which were not taken originally to investigate the point under discussion. They are the result of a number of different experiments and, therefore, are not perfectly consistent. It is believed, however, that they do in general illustrate the fact that in narrow channels the high velocities come closer to the sides.

The writer is unable to accept Mr. Tchikoff's arguments regarding the velocity gradient. He states: "The sum of the potential and kinetic energy is constant for all the points of the stream filament that lie on the same horizontal plane." This would only be true in a frictionless channel Where friction is present, the water particles near the sides of the channel are more retarded by friction and more of their energy is converted into heat, than those near the center. On a given horizontal plane, therefore, the sum of the potential and kinetic energy of the water at the sides of the channel may be less than near the center. This is demonstrated by measurements made in a chute near Montrose, Colo.[52] A velocity of 34.8 ft per sec was observed 0.4 ft below the surface near the center of the channel, and, at the same depth, at the sides velocities of 23.3 and 26.9 ft per sec were observed.

The water surface was level, and, hence, the pressure at the three points was the same. The kinetic energy, expressed in velocity head, at the sides was 10.4 and 7.6 ft, respectively, less than at the center. At another section of the same chute (not described in the paper cited[52]), float measurements showed a velocity of 20 ft per sec at the center and about 9 ft per sec as close to the sides as could be observed. The water surface was shown by measurements to be level, and the pressure was the same (atmospheric), but the sum of the potential and kinetic energies was 5 ft of head greater at the center than near the sides. By observations at a large number of cross-sections the water surface was shown to be level across the flume (except at the entrance where contraction effects entered), but the water in the center was obviously moving much more rapidly than near the edges and, therefore, the sum of the potential and kinetic energies at the center was greater than at the sides. The error in Mr. Tchikoff's statement arises from not considering the friction term in the Bernoulli theorem.

Mr. Blaney has called attention to the difficulties found in his investigations of the Lower Colorado River, in analyzing the data, because of the widely different size of silt particles carried. In the writer's studies made for the U. S. Bureau of Reclamation in 1934 and 1935, on the silt carried in the Colorado River, a discovery was made which is believed to be of great significance in understanding the transportation of silt in suspension. It has long been known that, in many rivers (including the Colorado) carrying heavy silt charges, there is no close relation between the discharge and the total silt load. In these studies analysis was made not only of the quantity but of the size range of the particles carried in the Colorado River, by means of bi-weekly samples taken over a period of more than a year. It was found that although there was no relation between the quantity of fine silt and the discharge there was a very definite relation between the load of coarser particles and the discharge. Some of the results of this study have been published.[51] The size range of

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[52] "Recent Studies on Flow Conditions in Steep Chutes," *Engineering News-Record*, Vol. 116, January 2, 1936, p. 5.

the particles the load of which bore a definite relation to the discharge was the same as the range of sizes found in the bed material. The reason for this condition is believed to be the fact that the river, in general, always carries as great a load as it is able, of the material which is available in large quantities in its bed. Because of the eddies which impinge on the river bottom, particles from the bottom are thrown into suspension. At the same time, other particles from suspension are settling down again on the river bed. When the same number of particles are picked up as settle back to the bottom the load is as great as the river can carry. When fewer are picked up than fall back, the bed is being filled and when more are picked up the bed is being scoured. With a given size of material on the river bed, the river will pick up sufficient material to produce a condition in which the rate of re-depositing on the bottom is equal to the rate at which the material is picked up from the bottom. The load of very fine silt which the river is capable of carrying seems usually to be much greater than the supply available, which is limited by the quantity of fine material that is brought into the stream by its tributaries. The load of fine material, therefore, is dependent entirely on the load brought to the stream by the tributaries and, hence, is independent of the discharge carried by the stream. The stream picks up from the river bed all the fine material available, but is not able to obtain from this source sufficient material to saturate it with the finer particles. It is believed that many other of the phenomena of the transportation of silt in suspension may be cleared up as a result of this discovery, and that it can form the basis of a quantitative science in this field.

The dimensions adopted for the All-American Canal have been requested and are given in Table 14.[53]

TABLE 14.—ALL-AMERICAN CANAL CROSS-SECTIONS*

Discharge, in cubic feet per second	Bottom width, in feet	Depth, in feet	Side slopes 1 on 1.75, etc.	Area, A, in square feet	Wetted perimeter, P, in feet	Hydraulic radius, R,
15 155	160	20.61	1.75:1	4 041.3	243	16.63
13 155	150	19.12	1.75:1	3 508.0	227	15.45
10 155	130	16.59	2:1	2 708.0	204.2	13.26
7 600	118	14.73	2:1	2 171	183.8	11.81
7 400	116	14.57	2:1	2 114	181.2	11.67
7 100	114	14.24	2:1	2 029	177.6	11.42
6 800	114	14.02	1.75:1	1 943	170.5	11.39
5 100	100	12.82	1.75:1	1 569	151.6	10.34

*Earth sections only.

In selecting these sections several factors were unusually important. One was the great depth of cut in which much of the canal was built, which reached a maximum of more than 100 ft and for long stretches was more than 75 ft. Another was the fact that considerable time may elapse before the demand reaches the design capacity. Both these factors justify the use of narrower widths and higher velocities than would otherwise be the

[53] Specifications 573 and 621, U. S. Bureau of Reclamation.

case. In comparing this canal with the Imperial Valley canals it should be kept in mind that the material traversed by them is not the same. The Imperial canals, in general, flow through a Colorado River deposit of very fine material, whereas the All-American Canal is nearly all located on higher land in coarser "mesa material" which seems to be an outwash deposit from the surrounding high land.

In the acknowledgments accompanying the author's paper he inadvertently omitted mention of his indebtedness to Mr. W. M. Griffith, to H. F. Blaney, and F. C. Scobey, Members, Am. Soc. C. E., and to the Central Board of Irrigation of India, for valuable data and suggestions.

AMERICAN SOCIETY OF CIVIL ENGINEERS

Founded November 5, 1852

DISCUSSIONS

ADMINISTRATIVE CONTROL OF UNDERGROUND WATER: PHYSICAL AND LEGAL ASPECTS

Discussion

By Harold Conkling, M. Am. Soc. C. E.

Harold Conkling,[47] M. Am. Soc. C. E. (by letter).[47a]—The able and instructive discussions of the subject-matter of this paper "bring home" the considerable thought which is being devoted to the subject by hydrological engineers. The paper and the discussions do not deal with a matter that has been finished, nor with a mathematical certainty, nor with the results of an experiment; rather they deal with what may be the difficulties in, and what may be the desirability of, administrative control of a natural resource, the limitation on the use of which is obscure and is enwrapped with questions of economics. In such discussions informed opinion naturally plays a large part.

The discussions have indicated at least a part of the errors of commission and omission in the original paper. The writer is grateful for this, and it is hoped that all readers will note them. Any errors in acreage irrigated by pumping are not at all material to the purpose of the paper. These data are from the Federal Census which is the only source available, as was found by the writer after attempting to obtain the same information from the various State offices. In the discussions the scope of the paper has been amplified to the benefit of all. It was hoped that both correction and amplification would be the outcome. "Consider," said Prah-hotep 5 000 yrs ago, "How thou mayest be opposed by

Note.—The paper by Harold Conkling, M. Am. Soc. C. E., was published in April, 1936, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: August, 1936, by Messrs. Joseph Jacobs, and W. D. Faucette and J. E. Willoughby; September, 1936, by R. E. Savage, Assoc. M. Am. Soc. C. E.; December, 1936, by Messrs. H. J. F. Gourley, and O. J. Baldwin; January, 1937, by Messrs. G. E. P. Smith, and David G. Thompson; February, 1937, by Wells A. Hutchins, Esq; March, 1937, by Messrs. John E. Field, George S. Knapp, and Chandler Davis; and April, 1937, by L. Standish Hall, Assoc. M. Am. Soc. C. E.

[47] Deputy State Engr. of California, Sacramento, Calif.

[47a] Received by the Secretary March 31, 1937.

an expert in council. It is foolish to speak on every kind of work;" and said Tai Tung, "Were I to await perfection my book would never be finished."

Many lines of thought have been suggested in the discussion which, however tempting, would lead to an inordinately long closure, and because of this specific mention is made of only a few of the factual discussions of conditions in specific areas.

That of Mr. Gourley gives light on conditions in England where the development of modern industry is bringing to the fore the desirability of the control of underground water use. Mr. Baldwin has also called attention to industrial developments in the Mid-Western United States which show that even there a problem now exists. The discussion of Professor Smith describes in detail conditions in Arizona and neighboring States. In this connection a *Bulletin* by Professor Smith[48] on ground-water law, published in 1937, is well worth reading. Mr. Thompson has supplemented and amplified the original paper by a broad and masterly outline of conditions in the eastern part of the United States and by an analysis of legal decisions in both the West and the East. A discussion of conditions in Colorado, by Mr. Fields, is illuminating because of the intensive development of water supplies in the eastern part of Colorado. This latter discussion appears to support the writer's thesis that in a State having the appropriative doctrine, use of underground water would be legally inhibited by surface rights below the point of use as long as the concept prevails that surface flow is necessary to fill a right which, heretofore, has depended on surface flow. The successful pumping which has been instituted along the South Platte River in the recent years of drought, has been successful because of "the live and let live attitude of canal administrators who have been adversely affected, has permitted their use."[49] In other words, development of ground-water succeeded, and the crops of the area were preserved in the recent stressful years because the law was not invoked. This is cogent commentary on the difficulties that arise in the complete application of the law of prior appropriation when ground-water is involved. The foregoing statement, however, seems somewhat contradictory to a subsequent statement in the same discussion to the effect that the prior appropriator is not protected in his means of diversion and may be forced to pump. Perhaps Mr. Fields did not have in mind pumping from under-flow as an alternative means of diversion.

All who have discussed the paper unite as to the need of administrative control. To the writer this seems clear also, but he is not so definite in his belief that actual control is needed at an early stage of development. A ground-water basin in its original state contains large deposits of water which, in most cases, can be withdrawn without great harm

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[48] "Ground Water Law in Arizona and Neighboring States", by G. E. P. Smith, M. Am. Soc. C. E., *Bulletin No. 65*, Coll. of Agriculture, Tucson, Ariz.

[49] *Proceedings*, Am. Soc. C. E., March, 1937, p. 549.

except that the water-table is lowered and the permanent supply is thus rendered more expensive to secure. In this respect, it is not different from an ore body or an oil field. If these deposits of water are not exploited, the potential wealth of the country is not developed; and yet, if they are exploited, it means that some of those using the water during the period of exploitation must cease using it at some future period. In other words, these users have only a temporary right; but it seems to violate a traditional concept to speak of a temporary right in water. How valuable this exploitation may be in some cases is witnessed by development in Southern California and in the San Joaquin Valley of California where, with mining of water as a foundation and mining of oil as a supplement—in the fields side by side with the water basin—wealth has been produced which has enabled or is enabling the communities to construct huge aqueducts to import water from sources hundreds of miles distant to replace the mined water and to bring in supplies much greater than the permanent local supplies.

An uncontrolled development may bring about the best development or it may not; but whatever the result the means of its achievement are costly. Administrative control at its best would bring about the best result in the least costly manner, but it cannot depart too widely from the natural trend of events. If a temporary over-development which mined the impounded water supplies of the past ages seemed best for the public it would be permitted and, therefore, permanent and temporary rights would exist. The same results would thus be achieved finally as are achieved in many cases by uncontrolled development; that is, temporary over-draft and final adjustment of draft to recharge, with resulting abandonment of part of the development. The difference in such case would be that the temporary rights would be designated in the case of control, whereas, with lack of control, such rights would be those of the economically marginal users instead of the last users.

The point is that the need for flexibility in administration is paramount. The principle of prior appropriation, if actually administered according to the doctrine, can be very inflexible. The doctrine of correlative rights can be very flexible. Either doctrine may lead to beneficial results or either may lead to harmful results. In contemplating possibilities, the writer confesses to a distinct distaste for "freezing" any situation within the narrow confines of administrative action and control unless the administrator can be given the right to accept or to reject the areas to be placed within his control, either by his own initiative or by action on a petition of those using water. As an alternative the administrator can be given a power he is not now given in any jurisdiction; namely, the exercise of judicial functions.

Mr. Thompson's discussion brings out the apparent necessity felt by the Courts to define classes of underground water so that each situation falls into a certain category as to which a precedent exists or as to which a new precedent can be set. Even by a hydrologist, the artificiality of

such definitions can be appreciated only when he must examine them in detail. Recently, the writer was faced with the necessity of deciding, for purposes of determining jurisdiction, whether a certain body of water was a "definite underground stream," or otherwise. Any reasoning a hydrologist could bring to bear on the matter which led to the conclusion that this body of water is an underground stream, would also lead to the conclusion that almost any moving underground water is an underground stream, and *vice versa*; but the first-mentioned reasoning process seemed the soundest. The writer is heartily in accord with Mr. Thompson's axiom that "all water beneath the ground is * * * simply ground-water." The difficulty is that the definitions and the need for definitions have been due to lack of knowledge, with little appreciation of the broader phases of the matter.

To summarize: The numerous and able discussions have done much to clarify the subject. It is felt that control is desirable, but that it can be accomplished best through a step-by-step process whereby the results of each step can be considered before going further. The attempts at control in the various jurisdictions and in England have been outlined. Of course, they are first steps, but it can scarcely be concluded after reading these discussions that a uniform law is desirable. Contrast conditions in Colorado as described by Mr. Field, with conditions in California as typified by San Gabriel Valley; or contrast with these, conditions in Long Island as described by Mr. Davis. Nevertheless, the basic ideal would necessarily be the same no matter how diverse the form of the law; and by it questions of draft which would cause final retrogression would be encompassed whether the retrogression were caused by marine or other saline intrusion, by too great concentration of chemicals, or simply by drawing more water from the basin than the recharge permits.

If engineers feel that control is desirable they must formulate the kind of control. This matter has become, in some areas (and will become in others), a matter which ranks with other phases of life which have been given into the control of commissioners with judicial or quasi-judicial power. If the development of underground water brings such a situation about as to its control, it will naturally extend to surface water since the two are physically interdependent. It is suggested that such a body, combining in its personnel, both scientific knowledge and ability for investigational research, can most intelligently cope with the present problem, subject of course, to review by the Court.

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DISCUSSIONS

BACK-WATER AND DROP-DOWN CURVES FOR UNIFORM CHANNELS

Discussion

BY P. CHARLES STEIN, ESQ.

P. CHARLES STEIN,[18] ESQ. (by letter).[18a]—The method for determining back-water and drop-down curves in uniform channels, described by the author, lends itself to a relatively simple routine for computing these problems. The errors introduced by the approximation that the area and the wetted perimeter are monomial functions of the depth are of small order and should be well below the range of error incident to the selection of a friction factor.

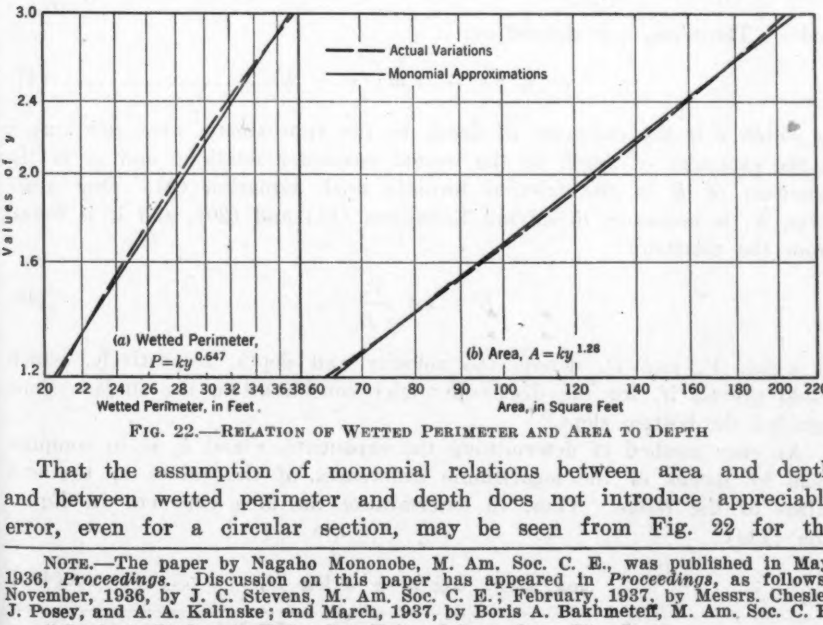

FIG. 22.—RELATION OF WETTED PERIMETER AND AREA TO DEPTH

That the assumption of monomial relations between area and depth and between wetted perimeter and depth does not introduce appreciable error, even for a circular section, may be seen from Fig. 22 for the

NOTE.—The paper by Nagaho Mononobe, M. Am. Soc. C. E., was published in May, 1936, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: November, 1936, by J. C. Stevens, M. Am. Soc. C. E.; February, 1937, by Messrs. Chesley J. Posey, and A. A. Kalinske; and March, 1937, by Boris A. Bakhmeteff, M. Am. Soc. C. E.

[18] Graduate Student in Hydraulics, Univ. of Iowa, Iowa City, Iowa.

[18a] Received by the Secretary March 16, 1937.

example given by Professor Mononobe (see text following Equation (32)), in which the depth in a circular conduit, 18 ft in diameter, ranged from 30% to 75% of the diameter. The heavy dashed lines in Fig. 22 show the actual variation of area and wetted perimeter and the full lines represent the monomial approximations.

It is to be noted that the range in depths of a back-water curve cannot extend across the critical depth. In practical problems, even in channels not well adapted to the monomial approximation over their entire range, the range will usually be such that the assumption of monomial variation will agree closely with the actual variation of the hydraulic elements with respect to depth.

Removed from its seemingly complex mathematical treatment, the author's method involves very simple reasoning and is adapted to a relatively easy computing procedure. Having expressed the area and the wetted perimeter as approximate exponential functions of depth, Professor Mononobe converts the differential equation of Bernoulli's theorem for a sloping channel (Equation (1)) into a form in which all hydraulic variables are functions of depth and, after grouping terms, integrates the resulting equation to obtain Equation (18) which defines the back-water curve in a uniform channel.[18b] A similar formula, Equation (20), defines the drop-down curve.

Equations (18) and (20) are solved readily by means of the curves in Figs. 6, 7, and 8. The parameters necessary for using the charts are $\frac{D}{D_0}$, r, and s. The term, r, is defined as:

$$r = 2s + 2m\,(s - k) \qquad \text{(47)}$$

in which s is the exponent of depth in the approximate area relation; k is the exponent of depth in the wetted perimeter relation; and m is the exponent of R in the friction formula used, Equation (2). One other term, K, is necessary in solving Equations (18) and (20), and it is found from the relation:

$$K = \alpha\, s\, \frac{V_0}{g\, D_0} \qquad \text{(48)}$$

in which V_0 and D_0 denote the velocity and depth, respectively, which would prevail if, for the discharge under consideration, the surface slope equalled the bottom slope.

An easy method of determining the exponents, s and k, is to compute them by means of the logarithmic differences of the values at the end points of the range. Thus, in determining the area exponent in Equation (13*a*):

$$\log A_2 = \log a + s \log D_2 \qquad \text{(49a)}$$

[18b] Correction for *Transaction:* In Equation (18), $\Phi_2 = \int \frac{y^{(r-2s)-1}}{y^r - 1}\, dy$.

and,

$$\log A_1 = \log a + s \log D_1 \qquad (49b)$$

Subtracting, and solving for s:

$$s = \frac{\log A_2 - \log A_1}{\log D_2 - \log D_1} \qquad (50)$$

For the circular conduit mentioned in the text following Equation (32): $A_2 = 204.7$; $A_1 = 64.2$; $D_2 = 13.5$; and $D_1 = 5.4$. Reading the logarithms on an ordinary slide-rule: $s = \frac{2.311 - 1.807}{1.130 - 0.732} = 1.267$. Similarly, for the wetted perimeter, this type of calculation results in: $k = 0.646$.

It is seen that the exponents thus obtained agree closely with those derived by the graphical method. In the graphical method, the eye is guided in large measure by the location of the end points.

The author's method, wherein he assumes that the average of the slopes at the ends of the range is equivalent to the slope of the straight line best approximating the curve, is open to criticism inasmuch as it may give undue weight to an extreme slope at one end of the range. The logarithmic method is simpler than the method given in the paper and is probably less subject to error.

AMERICAN SOCIETY OF CIVIL ENGINEERS

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DISCUSSIONS

DYNAMIC DISTORTIONS IN STRUCTURES SUBJECT TO SUDDEN EARTH SHOCK

Discussion

By Harry A. Williams, Assoc. M. Am. Soc. C. E.

Harry A. Williams,[14] Assoc. M. Am. Soc. C. E. (by letter).[14a]—It was the intent of the writer to show that, given a near resonance condition, the vibratory amplitudes of single-mass systems build up to critical values quite rapidly. Experimental evidence was offered in support of the mathematical theory. The theory was then simplified and applied to an elevated water tank. This was not done with the idea that all future elevated tanks or other single-mass structures should be designed only in accordance with the simplified equation presented by the writer. It was offered rather as another approach by which an engineer might check his designs and guide his judgment.

Discussions were presented by Messrs. Ruge, Engle, White, and Jacobsen. Professor Ruge further substantiated the writer's contentions. His comments on the writer's conclusion regarding the desirability of designing a structure with a period different from that of an expected earthquake are well taken. The writer's original statement was made with the hope that in the distant future the type of local ground motions might be reasonably predictable. Only a study of the records of many future earthquakes will answer this question. Meanwhile, it is doubtful whether an elevated water-tank structure can be designed along conventional lines and not be in resonance with any dangerous ground motions. Designing the structure so that the elastic constant varies with the amplitude might be a solution to the problem. Certainly the method of adding more steel to stiffen the structure is open to the criticism offered by Professor Ruge.

Mr. Engle presents the so-called "static" point of view and in so doing raises a number of questions with which the writer finds himself

Note.—The paper by Harry A. Williams, Assoc. M. Am. Soc. C. E., was published in May, 1936, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: September, 1936, by Messrs. Arthur C. Ruge, and H. M. Engle; February, 1937, by Merit P. White, Jun. Am. Soc. C. E.; and March, 1937, by L. S. Jacobsen, Esq.

[14] Asst. Prof., Civ. Eng., Stanford Univ., Stanford University, Calif.

[14a] Received by the Secretary April 15, 1937.

in disagreement. A few of these are as follows (the quotations are from Mr. Engle's discussion):

(1) "There has been a tendency on the part of those interested in shaking-table experiments and so-called 'dynamic design' to consider the behavior of these structures [elevated water tanks] as somewhat of a mystery, requiring complicated model study and theoretical experimentation." Knowing that the behavior of a vibrating structure cannot be explained by the static method, those interested in the dynamic viewpoint are inclined to approach such a problem with some humility and study the reactions of a structure to simplified motions rather than attempt to explain its actual behavior during an earthquake by reasoning *a priori* from theories having very little, if anything, to do with the problem. Mr. Engle states, furthermore, that "there is no particular mystery involved" in the behavior of these structures, and that "a reasonable explanation of the damage can be made." Yet he asserts in a later paragraph that the problem is extremely complicated.

(2) To quote further: "At present there is no definite proof that the static method of analysis involves any more assumptions or uncertainties than the so-called dynamic method." Obviously, it is necessary to make assumptions in applying the dynamic method to practical problems. The writer made a number of these assumptions in his paper when he investigated the behavior of the elevated water tank. He also showed how there could be considerable variation in many of the assumed conditions and yet the results would remain fairly indicative of the probable behavior of the structure during a short interval of an earthquake; and he warned the reader against drawing quantitative conclusions from such an investigation. In the static method, a fictitious force is applied to the center of gravity of the structure, this force being made equal to the mass of the structure times an arbitrarily assumed acceleration. Only by sheer coincidence, as emphasized in Professor Jacobsen's discussion, will it give stresses which are indicative of the actual stresses which might be expected. It would be just as logical a procedure to design the tank structure for a 200-mile wind. In doing this, one would be candidly admitting that he is merely applying an empirical formula to his problem. The writer, of course, is not advocating that the wind-force method be substituted for the static method. The latter is a least a step in the right direction, and it is making designers "mass conscious."

(3) "A static analysis of damaged towers shows that they should have failed where failure occurred." No doubt this was true, yet it does not justify the static method, because this method only divulges the weak points in the structure. If a collapsed tower is replaced with a structure which is identical except that the weaker points are strengthened, another earthquake of no greater intensity than the first may easily wreck the new tower.

The structural engineer has always been trained to design for strength. If a structure failed because of the weakness of a particular member,

that member was strengthened in the new structure. The mechanical engineer designed in the same way until he was confronted with the development of high-speed machinery. He then discovered that it was sometimes better—as in De Laval's steam turbines and cream separators—to make a shaft more flexible rather than stronger and stiffer in order to avoid resonance and large vibrations at operating speeds. It is true that in some cases an equally acceptable result could have been attained by using a heavier shaft, but this would have required very accurate workmanship and, consequently, would have materially increased the cost of the machine. In the same manner, certain structures can be made quite safe if they are stiffened sufficiently, but this is economical and practicable only in certain cases.

(4) During the Long Beach earthquake, the top panel rods in many tank structures were badly stretched or entirely broken. The writer suggested that possibly many of these failures occurred soon after the beginning of the major shock, because of a near-resonance condition. Mr. Engle strongly disagreed with this contention, mainly because such action was "not consistent with the failure of steel tension members" since "stretching takes place first and it takes an appreciable time to jerk the rods apart." The elementary theory of dynamics and strength of materials should make it apparent that the dynamic forces take time to stretch the rods of such a structure. The length of time taken depends on the velocity of the mass center of the structure at the instant the elastic limit is reached in the top panel rods. As soon as this critical deflection is reached the tower becomes a structure with radically different dynamic and elastic properties. It may not return to its original position for several seconds. It seems to verge on the miraculous that "the cessation of the shock was all that saved many towers [with badly stretched or broken rods] from collapse"—not to mention the chimney of the Seal Beach Power Plant with broken diagonals in its supporting steel structure. It appears improbable to the writer that each tower was subjected to a motion which stretched or broke its top panel rods and then stopped just in time to prevent collapse. In evaluating the effect of slack rods picking up their loads with a "jerk," it should be remembered that a flexible tower may not necessarily have very high velocities engendered by the ground motion. Moreover, the nature of the "jerk" is in reality a gradually applied elastic force.

There are a number of other points presented by Mr. Engle that could be discussed. It is agreed that the behavior of elevated tanks in recent earthquakes has been surprisingly good. Only further study and earthquake experience, combined with a rational dynamic point of view, will divulge how much present design can be improved.

The writer does not believe that the dangers from resonance or near resonance have been exaggerated. Chaotic motion does alleviate some of the danger, but evidence is reasonably conclusive that large distortions do build up rapidly, at least for single-mass systems, and records show

that the ground does follow a more or less regular motion for a few cycles at a time. Shaking-table experiments made at Leland Stanford University and at the Massachusetts Institute of Technology leave little doubt but that a vessel and a part of the liquid it contains act as a single-mass system for a few cycles. Wave action, although having some "cushioning effect" at once, is most noticeable later. Mr. Engle's remarks concerning resonance apply if one is thinking of the general behavior of the elevated-tank structure throughout the major part of a shock. They do not detract from the writer's contentions.

Mr. White's emphasis and elaboration of certain points of the paper are interesting. It is important to note, as he indicates, that the region of dangerous frequencies is somewhat wider in the case of suddenly applied motion. This point further emphasizes the difficulty of avoiding near resonance by making only nominal changes in a single-mass system subject to earth shock.

The writer is very much indebted to Professor Jacobsen, not only for his comments on the paper, but also for the addition of a very desirable discussion pertaining to buildings. The last line of Table 1 shows how much in error one might be if he were guided entirely by the static method. As Professor Jacobsen points out, the dynamic forces involved for the "below resonance" condition are of a large order, and they occur very shortly after the beginning of the ground motion. In comparing the "below resonance" and "near resonance" results, the reader may be interested in the data as it is arranged in Table 3 for the tests involving the least friction only.

TABLE 3.—DATA OBTAINED FROM FIGS. 2 AND 3

	Below Resonance		Near Resonance		Above Resonance	
(a) Ratio of model period to ground period	0.77		1.06		1.21	
(b) Relative values of elastic constants	2.4		1.3		1.0	
(c) Number of cycles of ground motion when the dynamic force occurs	1.1	1.8	1.9	2.4	1.5	2.0
(d) Ratio of dynamic force on model to weight of model	0.31	0.38	0.27	0.40	0.19	0.23
(e) Ratio of dynamic force to a constant fictitious force based on maximum harmonic acceleration of the ground, 0.085 g times mass of the model	3.7	4.5	3.2	4.7	2.3	2.7

These results show that relatively large dynamic forces are set up also for the "near resonance" condition during the first few cycles even if the maximum does not occur for some time. It will be noted that the dynamic force equals 38% of the weight of the model at the end of 1.8 ground-motion cycles in the case of "below resonance" whereas it reaches 27% in approximately the same time for the "near resonance" model. If the models represented two conventionally designed structures, there would be a question, depending on the design,

as to which structure was being subjected to the more critical unit stresses. The rigidity of one structure may be double that of another whereas its critical strength is only slightly greater. In other cases, the more rigid structure may be very much stronger.

The results of experiments with a 17-story building model, which Professor Jacobsen presented as a part of his discussion, further emphasize the effects of resonance, these effects becoming quite important in one or two cycles of ground motion.

It is agreed, as Mr. Engle stated, that "actual disaster experience where properly interpreted is a fairly good foundation for basing future practice." The proper interpretation of the results of "Nature's shaking-table experiments" is of the utmost importance. Although it is entirely possible that at some future time those interested in the dynamic approach will conclude that there are too many variables in most practical problems to justify the use of mathematical expressions based on the more exact dynamic theory and that an empirical approach similar to the existing static method must be resorted to, that time has not yet arrived. When and if it does arrive, those applying the empirical method will find that a familiarity with the behavior of structures when subjected to very simple motions will be an invaluable aid.

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DISCUSSIONS

ANALYSIS OF VIERENDEEL TRUSSES

Discussion

BY DANA YOUNG, ASSOC. M. AM. SOC. C. E.

DANA YOUNG,[45] ASSOC. M. AM. SOC. C. E. (by letter).[45a]—The discussions have revealed many interesting facts. Additional examples have been presented and checked, other methods of analysis have been recommended, and several "short-cut" procedures have been proposed.

Mr. Mensch has presented an instructive application of the method to a multi-storied bent. It may be noted that Equation (97) is simply a special case of Equation (53) and may be readily obtained from the latter. Table 1, which gives a comparison of H-values obtained by the general method with those from the approximate portal method, is helpful in showing the errors that may occur from using the latter method. Mr. Mensch has also given a "short-cut" method of applying the general equations to a specific problem. This is a good illustration of the simplifications that are practical in many cases, as was noted in the paper under the heading, "Special Approximations."

Mr. Eremin has suggested that, for trusses with parallel chords and constant moment of inertia, it is sufficiently accurate to assume points of contraflexure in all members at their mid-lengths; that is, the portal method. However, it is well known that this method is often in serious error. This fact is clearly illustrated by Mr. Mensch in Table 1. Equation (122),[45b] which is given without proof, is readily derived from statics, using the assumption that $\dfrac{M'_{ba} + M'_{bc}}{M''_{ba} + M''_{bc}} = \dfrac{I'_c}{I''_c} = \dfrac{I_t}{I_b}$. Although this assumption is satisfactory in some cases, it may be shown that in trusses with large differences in the moments of inertia of the chords, the error will be considerable. The approximate formulas, Equation (123) and

NOTE.—The paper by Dana Young, Assoc. M. Am. Soc. C. E., was published in August, 1936, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: November, 1936, by Messrs. L. J. Mensch, A. A. Eremin, Leon Blog, A. W. Fischer, and L. C. Maugh: January, 1937, by John E. Goldberg, Jun. Am. Soc. C. E.; February, 1937, by Messrs. Louis Baes, E. C. Ingalls and Ralph B. Peck, Harold E. Wessman, and A. Floris; and March, 1937, by Messrs. Kimball R. Garland, and J. D. Gedo.

[45] Asst. Prof. of Eng., Connecticut State Coll., Storrs, Conn.

[45a] Received by the Secretary April 13, 1937.

[45b] Equation (120) is evidently in error, since it follows from Fig. 19 and simple statics that $q_{b10} = \frac{1}{2} P_b$.

Equation (124), especially Equation (123), which Mr. Eremin proposes—are too inaccurate for general use. Although they give good results in the particular example given, they cannot always be relied upon to do so.

Mr. Blog has presented a summary of Professor Vierendeel's method of analysis and claims that this method is simpler and more accurate than that proposed in the paper. It is true that Equations (127) through (130), inclusive, are simpler than those in the paper, but that is because the former group is based upon several assumptions. Because of these assumptions, it is not true that the Vierendeel equations are as accurate as the others. The primary assumption in the Vierendeel method is that,

$$\frac{M'}{M''} = \frac{I'}{I''}\frac{dx}{ds} \qquad (155)$$

Mr. Blog states that Professor Vierendeel proves that this relation is true. However, a careful check of this derivation[9] will show that it is based on the assumption that the vertical deflection of corresponding points on the upper and lower chords of a truss are equal throughout the length of the truss. This is not true except in special cases. The fact that Equation (155) is not exact may be readily shown by a numerical example in which the moments of inertia of the chord members are not equal. Such an example has been very conveniently solved by Messrs. Ingalls and Peck in their discussion. An inspection of their results, which are given in Table 2, shows this immediately. Another assumption which Professor Vierendeel makes, as stated by Mr. Blog, is that the upper chord has a finite area but an infinitely small moment of inertia. As a result Equations (127) to (130), inclusive, involve these assumptions and, therefore, must be used with caution.

Messrs. Fischer, Goldberg, and Ingalls and Peck have added materially to the paper by developing other examples and checking by other methods of analysis.

Several of the discussions have indicated other methods of analysis. Mr. Maugh has described an interesting method using deformation equations with auxiliary force systems, which may be solved either directly or by successive approximations. This discussion contains a numerical computation which purports to show that an error of -3% in H_{1w} and of $+3\%$ in V'_{12} might cause an error of 40% in M'_{21}. This example neglects the relation that exists between H_{1w} and V'_{12} which is given by Equations (77) and (79). From these equations, it is seen that, for the condition assumed by Mr. Maugh (that is, $M_{1w} = 2H_{1w}$), the error in V'_{12} must be in the same direction as that in H_{1w} and, hence the error in M'_{21} will be much smaller than shown in his calculation. Professor Baes has referred to an ingenious method based upon the experimental determination of the point of contraflexure in each vertical member. This method appears to be very promising since experience in analysis tends to show that the position of the point of contraflexure in the vertical members varies

[9] "Cours de Stabilité des Construction," by Arthur Vierendeel, Tome IV, Louvain, 1920.

only slightly for considerable differences in the moments of inertia of the truss members. Professor Baes objects to the statement that the fundamental equations are based upon the principle of virtual work. Unfortunately, it is true that the names of the various principles in structural mechanics are not standardized throughout the world. If this were done, the mass of current engineering literature would be considerably easier to follow. In this particular case, it is believed that the designation used is consistent with general practice in the United States.

Messrs. Wessman and Garland have applied the moment distribution method to the examples given. As these discussers have shown, although there are certain troublesome details in applying this method to a truss with inclined chords, the method is perfectly possible. In this connection, it is to be noted that the general equations given in the paper may also be solved by successive approximations, as was indicated, and will be found to converge very rapidly. Professor Wessman has raised the question as to whether the analysis of the Vierendeel truss is not so sensitive to changes in sections of members, that it is impossible to obtain a final design in one or two trials. Although such a condition is a possibility, it has been the writer's experience that no difficulty occurs in this direction in actual cases. The writer heartily agrees with Professor Wessman that analysis and design go hand-in hand" and must be considered together to insure satisfactory results, as in all good engineering.

Mr. Floris has stated that the Vierendeel truss is fundamentally inferior to trusses with diagonals. Although it is outside the scope of the paper to enter an extended argument as to the utility of this type of construction, it may be stated that the most usual form of Vierendeel truss for bridges is the bowstring type, which is very similar to a bowstring or tied arch, and possesses the advantages of this form of structure. In such cases, as mentioned by Mr. Maugh, the Vierendeel truss arrangement is often a simpler design, particularly with welded steel or reinforced concrete construction.

Mr. Gedo has presented an interesting and helpful discussion. His comparison of stresses in Vierendeel, riveted, and pin-connected trusses is very instructive. However, as mentioned previously, it would be more appropriate to compare the Vierendeel truss with a tied arch design, in order to understand best the possibilities of this form of construction. Mr. Gedo provides a refreshing contrast to some of the other discussers in that he advocates the direct solution of the general equation.

In closing, the writer wishes to express his sincere thanks to all the discussers for their contributions to this study.

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DISCUSSIONS

SIMULTANEOUS EQUATIONS IN MECHANICS SOLVED BY ITERATION

Discussion

By Messrs. A. Floris, and W. L. Schwalbe

A. Floris,[24] Esq. (by letter).[24a]—This illuminating paper will undoubtedly bring to the attention of engineers in practice the great importance of iteration in the analysis of statically indeterminate structures. This method of solving simultaneous equations of statics can be applied equally well to other problems than those treated by the author.

The use of converging approximations for determining the values of the end moments of beams and columns in the moment distribution method is, in reality, an iteration process. Furthermore, the treatment, by this method, of problems involving side-sway yields simultaneous equations the number of which is equal to the number of stories. These equations can also be solved by iteration[25].

The author treats the problem of side-sway in rigid frames by applying the equations of five angles. In these equations the unknown angles are determined by iteration. The writer's practice[25] in using the moment distribution method in case of side-sway is, first, to determine the coefficients of proportionality in the equilibrium equations by iteration. The values of the moments are then found by multiplying the distributed moments by these coefficients. In every case of lateral forces or unsymmetrical loads there are obtained simultaneous equations in which the coefficients of the diagonal falling from left to right are considerably greater than the remaining coefficients. In this case, the calculated values approach the true values by repeated approximations. In other words, the equations converge. Contemporary contributions to this important criterion of convergence are: Runge, (2)[25a], and Mises and Pollaczek-Geiringer (4) whose work has been cited by the author, and Helmut Wittmeyer[26].

Note.—The paper by W. L. Schwalbe, Esq., was published in August, 1936, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: November, 1936, by Messrs. Garrett B. Drummond, and A. W. Fischer; December, 1936, by M. B. Gamet, Jun. Am. Soc. C. E.; and January, 1937, by Messrs. Marvin A. Gray, and John E. Goldberg.

[24] Dipl.-Ing., Los Angeles, Calif.

[24a] Received by the Secretary January 6, 1937.

[25] "Analysis by Moment Distribution Aided Through Use of Iteration", by A. Floris, *Engineering News-Record*, June 25, 1936, p. 922.

[25a] Numerals in parentheses refer to Appendix I of the paper.

[26] "Ueber die Lösung von linearen Gleichungs-systemen durch Iteration", von Helmut Wittmeyer, *Zeitschrift für angewandte Mathematik und Mechanik*, October 1936, p. 301.

W. L. SCHWALBE,[27] ESQ. (by letter).[27a]—The procedure for solving n simultaneous equations of a general nature by successive approximations has been outlined by Mr. Drummond. The iteration procedure in the paper is more useful for those special systems that can be expressed by means of single equations, the single equation stating precisely what the iteration procedure must be for the solution of the n equations.

The example given by Mr. Fischer is a useful one for making a direct comparison of values computed by his exact method, by the procedure in the paper, and by the moment-distribution method according to Messrs. Thompson and Cutler.[6]

The development of "slope-deflection" methods in the United States proceeded essentially as pointed out by Messrs. Gamet and Goldberg. An additional reference may be cited, namely, the work of F. E. Richart, M. Am. Soc. C. E.[28]

In Germany, formulas of the type of Equations (22) and (23), Appendix II, were used by F. Lippich,[29] in the derivation of the three-moment equation. His equations included terms due to settlement of supports. F. Grashof[30] used formulas of the type of Equations (24) and (25) in deriving the three-moment equation for level supports. In his equations the load factor was not expressed in terms of fixed-end moments.

The writer would extend Mr. Gray's suggestion for a "symposium' to include not only "slope deflection," but all the theories of statically indeterminate analysis, with a glossary of symbols and terminology and a bibliography. In this connection the brief review and history, by H. M. Westergaard, M. Am. Soc. C. E.,[31] of the four general theories would serve as a basis.

The purpose of the paper (as stated in the "Synopsis") was not to present a new theory of statically indeterminate beams and frames, but to demonstrate the method of iteration in the solution of certain types of sets of simultaneous equations which occur in the application of theories already in existence. Consequently, the list of references is not a "who's who" in any particular theory.

It seemed desirable to extend the terminology which was begun by the introduction of the now well-established term, "three moment," to other equations of a similar character; hence, the use of the terms, "three angle" and "five angle" (3)[3]. To call equations of this type "slope deflection" equations, is a misnomer since "slope deflection" formulas are originally of the type of Equations (22) and (23). The latter are only

[27] Asst. Prof. of Theoretical and Applied Mechanics, Univ. of Illinois, Urbana, Ill.

[27a] Received by the Secretary April 12, 1937.

[6] *Transactions*, Am. Soc. C. E., Vol. 96 (1932), pp. 108–110.

[28] "Statistically Indeterminate Stresses in Stiff Framed Structures," by F. E. Richart, presented to the Univ. of Illinois, 1915, in partial fulfillment of the requirements for the degree of Master of Science in Civil Engineering.

[29] "Theorie des Kontinuirlichen Trägers Konstanter Querschnitte," *Allgemeine Bauzeitung*, Vol. 36 (1871), p. 104.

[30] *Zeitschrift*, Vereines Deutscher Ingenieure, Vol. 3 (1859), p. 155.

[31] "One Hundred Fifty Years Advance in Structural Analysis," *Transactions*, Am. Soc. C. E., Vol. 94 (1930), p. 226.

[3] Numerals in parenthesis refer to Appendix I of the paper, in August, 1936, *Proceedings*, p. 870.

a part of the story of statically indeterminate analysis. Virtual work or continuity and equilibrium conditions are also necessary to determine the final relationships.

The rapidly growing literature in the field of structural analysis gives one the impression of "schools of thought." Perhaps an increasing number of such esoteric sects is necessary for the growth of a subject but, surely, for a beginner the learning of fundamental principles underlying any later ritual is most important.

The importance of iteration in analysis has been emphasized by Mr. Floris. This procedure is especially useful when the statically indeterminate unknowns are selected in such a way that the set of simultaneous equations can be represented by a single equation in the form of a difference equation or recursion formula. Any unknown is thus expressed in terms of its neighbors. With known boundary conditions and convergence, the unknowns can then be computed by starting with any set of arbitrary values for the unknowns and correcting each value step by step by means of the recursion formula until all the values become fixed.

In the theory of elasticity, a differential equation can always be replaced by a difference equation which represents a set of algebraic equations. According to R. D. Carmichael[32]: "A differential equation, with or without boundary conditions, may be realized in an infinite number of ways as the limiting form of an algebraic system, so that there is always room for choice in setting up the system, and in fact need for care that it shall be done in a convenient way." The idea thus expressed may well serve as a guide for approximation methods in many problems for which integral solutions are impossible. The algebraic equations may be regarded as defining a statically indeterminate net or framework, the difference equation serving as a bridge from one field to the other.

Iteration as a method for the determination of roots of algebraic equations had its beginnings in 1674.[33] The earliest applications to the solution of problems in elasticity were probably those of C. Runge[34] and L. F. Richardson,[35] Since then the use of the method has been considerable.

[32] "Algebraic Guides to Transcendental Problems," *Bulletin*, Am. Math. Soc., Vol. 28 (1922), p. 184.

[33] "The Calculus of Observations," by Whittaker and Robinson, 1924.

[34] "Ueber eine Methode die partielle Differentialgleichung, $\Delta U =$ constant, numerisch zu integrieren," *Zeitschrift für Math. u. Physik*, Vol. 56 (1908), p. 225.

[35] "The Approximate Arithmetical Solutions by Finite Differences of Physical Problems Involving Differential Equations, with an Application to Stresses in a Masonry Dam," *Philosophical Transactions*, London, Vol. 210A (1910), p. 307.

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DISCUSSIONS

ANALYSIS OF CONTINUOUS FRAMES BY BALANCING ANGLE CHANGES

Discussion

BY MESSRS. P. CRAIG LIVESAY AND WILLIAM M. SIMPSON, AND L. E. GRINTER

P. CRAIG LIVESAY,[32] ESQ., AND WILLIAM M. SIMPSON,[32] ESQ. (by letter)[32a].—In opening his paper Professor Grinter has stated that the method of balancing angle changes has been presented as an additional tool to be used by the engineer wherever it may be found superior to other methods or an aid in checking a special problem. The relative merits of balancing angle changes as compared with other methods will depend upon the problem considered and the preference of the engineer who is making use of the method, which, again, will depend upon his familiarity with the various methods.

The writers have used the method of balancing angle changes to solve a variety of problems most of which have been checked by the method of balancing moments, or by the column analogy, in order to prove the self-checking features of the method. No difficulty has been experienced in obtaining results to any desired degree of accuracy, and no problem has been encountered to which this method could not be adopted. The types of problems solved include the following: Secondary stresses: bent analysis, involving side-sway; joint slip; wind-stress analysis in building frames; influence lines for continuous beams; and settlement of supports.

Because of the briefness of the treatment of side-sway by Professor Grinter, it was thought that an illustration of two special cases of joint translation would be useful.

The first example chosen to illustrate the method of handling side-sway is a slant-leg bent with an applied horizontal load. The analysis by

NOTE.—The paper by L. E. Grinter, Assoc. M. Am. Soc. C. E., was published in September, 1936, *Proceedings*. Discussion on this paper has been published in *Proceedings*, as follows: December, 1936, by Messrs. John E. Goldberg, G. A. Maney, Paul Andersen, and William F. Luce; January, 1937, by Ralph E. Byrne, Jr., Jun. Am. Soc C. E.; March, 1937, by Messrs. L. J. Mensch, Marvin A. Gray, and A. Floris; and April, 1937, by Messrs. Fred J. Benson, and David B. Hall.

[32] Teaching Assts., Dept. of Civ. Eng., Agri. and Mech. Coll. of Texas, College Station, Texas.

[32a] Received by the Secretary, April 9, 1937.

balancing angle changes is shown in Fig. 21. An unresisted side-sway similar to the one shown by the dotted lines has been assumed. (*E* has been omitted throughout, since it will cancel as may be shown by setting up the equations for the angle changes and the shears.) The angle changes

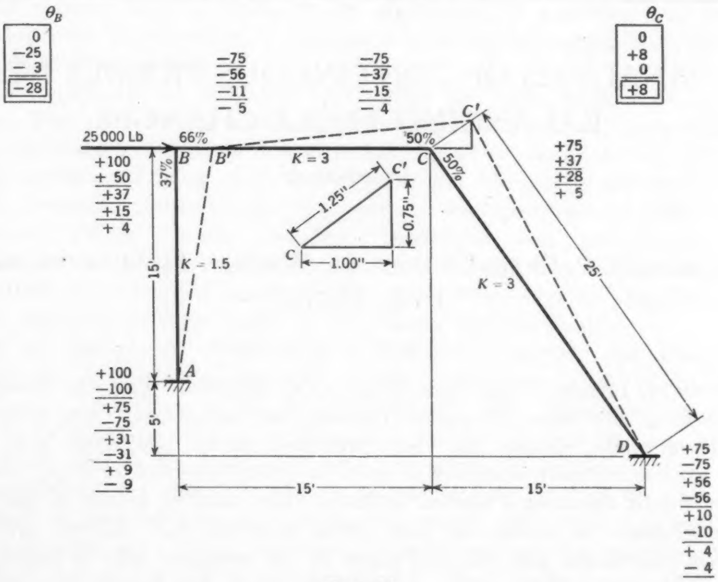

FIG. 21

were assumed as convenient values taken in proportion to the angles for a side-sway of 1 in. The small triangle above Joint *C* shows movements for a 1-in. deflection. These angles were then balanced by following the procedure outlined by Professor Grinter (under the heading, "Procedure of Balancing Angle Changes"). All joint rotations due to this balance are enclosed in rectangles near the joints. After the balancing of angles was completed, the horizontal shear in the vertical leg and the horizontal components of the shear and the direct stress in the slant leg were computed. The sum of these horizontal forces was found to 379 lb. The correction ratio was $\frac{25\,000}{379}$, or 65.9. The true joint rotations in the structure due to the horizontal loading were then found by multiplying the enclosed joint rotations by this ratio. The true ρ-values were found, similarly. This bent analysis illustrates the method of using a correction factor, as explained by Professor Grinter (see heading, "Bent Analysis").

The second example (Fig. 22) is an analysis of the frame presented by the author in Fig. 2, with the exception that the fixed support, *F*, has been replaced by a nest of rollers. Points *A* and *C* were assumed to lie in the same horizontal plane. This example illustrates the method of side-sway correction for an unsymmetrical structure carrying loads which are not

in the direction of free movement. The author's example permitted no side-sway because of the fixed support at F and thus no correction for side-sway was necessary.

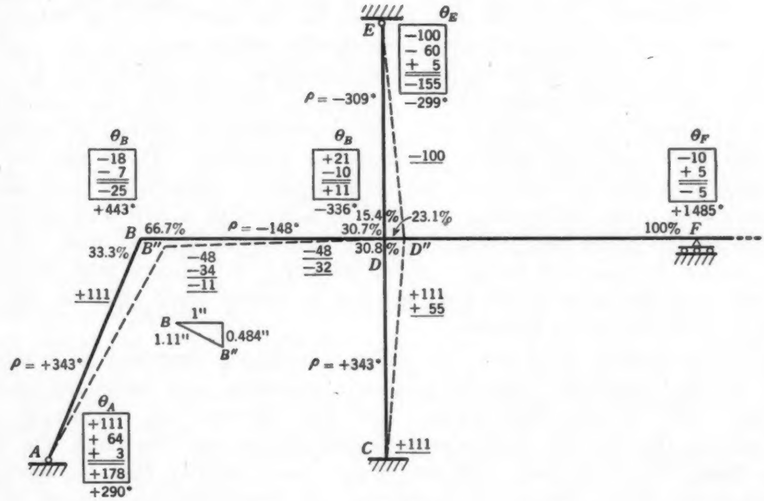

FIG. 22

When Joint F in Fig. 2 was replaced by a nest of rollers, the unbalanced restraint was found to be 1 044 lb. This restraint prevented side-sway which would have moved Joint D toward Joint F.

An unresisted side-sway was then imposed upon the pin-connected frame, as indicated by the dotted lines in Fig. 22. A rotation of − 100 was assumed in the member, DE. The geometrical deformation of the frame determined the rotations of the other members. The member rotations used in balancing angles were $\rho_{AB} = 111$, $\rho_{BD} = 48$, $\rho_{CD} = 111$, and $\rho_{DE} = -100$. All joint rotations due to this balance are enclosed in rectangles near the joints. The unbalanced horizontal force in the structure was found to be 338 lb. The modulus of elasticity has been neglected.

The joint rotations and the ρ-values of the assumed side-sway were multiplied by the ratio, $\frac{1\,044}{338}$. These corrected values were then added to the joint rotations indicated in Fig. 2 (multiplying the author's values by 10 before adding). Each final joint rotation and ρ-value is marked with an asterisk in Fig. 22. This completed the analysis for side-sway. In applying the slope-deflection equations to these values, the modulus of elasticity should be omitted.

When applying this method to particular problems many advantages and disadvantages, as compared to other methods, may be found. Regardless of the type of problem to which this method is applied there is one improvement over other methods. This advantage is its self-checking feature. There is a chance for compensating errors, of course, but that

chance is very slight. In addition, the method has been very useful to the writers in giving them a clearer picture of the physical deformation of a structure under any given set of conditions.

L. E. GRINTER,[33] ASSOC. M. AM. SOC. C. E. (by letter)[33a].—The unusually clear and useful discussions have undoubtedly added much to the clarification of the paper on balancing angle changes. As anticipated by the opening and closing paragraphs of the paper, the method has interested those who were either familiar with the slope-deflection method of analysis or who had used the method of balancing moments. It is not to be expected that others would find the method either immediately convenient or attractive, since they would not only be faced with a new and uniquely disturbing procedure but probably with a new terminology as well. However, many persons are being exposed regularly to contact with modern methods that are now in use by some one in nearly every office so that a gradual evolution is in progress.

It is interesting to observe the different opinions expressed with reference to the similarity between balancing moments and balancing angle changes Mr. Floris, Mr. Byrne, and Mr. Mensch are seemingly of the opinion that the two methods are practically one and the same. On the other hand, Mr. Gray and Professor Maney seem positive that the method of balancing angle changes is really the old slope-deflection method in a new dress. However, Mr. Goldberg answers adequately Professor Maney's contention that "'balancing angle changes' is merely a new group of words to describe what has been done for years in the solution of the slope-deflection equations," when he points out that "throughout the entire slope-deflection analysis the concept of continuity is firmly maintained," while in balancing angle changes "continuity is first destroyed and then * * * re-established." The resemblance that Professor Maney noticed was for a special case of secondary-stress analysis to which discussion the writer devoted only two sentences of the paper.

Several discussers, notably Mr. Floris, Mr. Goldberg, and Mr. Mensch had some difficulty in analyzing for side lurch by balancing angle changes. Mr. Floris will find that Mr. Andersen has solved this problem most excellently without the use of an "unresisted side lurch" whereas Messrs. Livesay and Simpson and also Mr. Benson apply, effectively and brilliantly, the conception of an "unresisted side lurch." The interesting problem of a two-level bent, analyzed accurately by Mr. Benson, will clarify the misconceptions of Mr. Goldberg that the method would necessarily become an approximation for a two-story frame. Evidently, the work involved in each of the examples presented by the discussers is much less than by any of the classical methods. The final moments can always be obtained to any degree of accuracy desired.

Mr. Andersen questions the writer's procedure of "carrying-over" from one joint before "balancing" at adjacent joints, as in Fig. 2. Experience

[33] Prof., Structural Eng., Agri. and Mech. Coll of Texas, College Station, Tex.

[33a] Received by the Secretary April 19, 1937.

with the several methods of successive corrections has shown that the procedure used in Fig. 2 produces the fastest possible convergence. The number of terms involved in Fig. 14 could probably be reduced considerably without the sacrifice of accuracy by adopting the balancing procedure from Fig. 2.

Mr. Gray stated in his opening sentence that the purpose of his discussion would be "to test the theory of this paper in all its technical phases." Since his discussion does not mention the use of balancing angle changes for any of the many purposes considered in the paper, it appears that this purpose was not carried to a conclusion. His discussion is composed mainly of a carefully presented demonstration that the ϕ-values and the carry-over factors can be derived from the slope-deflection equations. Naturally, this is true, but it is also true that these primary factors can be found directly by area moments, by virtual work, or by any of the known methods of deflection and slope computation. Mr. Hall's excellent discussion points out other procedures. It is worth noting that the slope-deflection equations are themselves derived by area-moments in most textbooks.

Mr. Luce has tested the method of balancing angle changes on a sufficiently wide variety of structures to speak with assurance as to its general application. The difficulty that he has found with reference to its use in wind-stress analysis has probably occurred to others; namely, that criterion ratios are based upon shears rather than angles and, therefore, a correction ϕ-value or ρ-value in a story is not easily determined. This difficulty can be overcome, however, by a study of the influence of an unresisted side lurch in a typical story upon adjacent stories when joint continuity is restored. This study will involve a correlation of ϕ-values and θ-values with story shears. An investigation of these relationships has been performed successfully under the writer's direction.

The thoughtful discussion of Mr. Byrne was pointed mainly toward the need for obtaining ϕ-values, stiffness factors, and carry-over factors for starting the process of balancing. The integral signs in his discussion give it a rather formidable appearance, particularly when it is compared with the paper in which the ideal of mathematical simplicity was paramount. However, Mr. Byrne does show conclusively how factors for moment and angle distribution are related. In answer to Mr. Byrne's questioning statement that he knows of no tables giving directly the quantities used for balancing angle changes, the writer submits Table 3 which was prepared during a study that he made in 1925. The source of these data is now unknown, although it is recalled that they represented only a slight revision of standard tables. They supply values of Factors A and B for substitution in the simplified formulas:

$$\theta = \frac{L\,A}{1\,000\,E\,I} \qquad \text{(42a)}$$

and,

$$\beta = \frac{L\,B}{1\,000\,E\,I} \qquad \text{(42b)}$$

in which θ and β, respectively, are the rotations at the left and right ends of a beam.

Mr. Hall may also be interested in Table 3, since the constants obtained from this table are for haunched beams to which his excellent discussion

TABLE 3.—FACTORS FOR SOLUTION OF EQUATIONS (42)

Values of $n^* = \frac{a}{b}$	Factors A and B, for the Following Values of $m\dagger = \frac{I}{I_c}$:												
	1.00	0.60	0.30	0.20	0.15	0.12	0.10	0.08	0.06	0.05	0.04	0.03	0.02
(*a*) STRAIGHT HAUNCHES													
0.5													
A	333	251	173	140	121	109	99	89	78	71	64	56	47
B	167	141	106	91	82	76	71	65	59	55	50	45	39
0.4													
A	333	264	198	171	154	144	136	127	117	112	106	99	91
B	167	147	125	115	108	104	100	96	92	89	86	83	78
0.35													
A	333	271	212	187	172	162	155	147	138	133	128	122	114
B	167	151	134	126	120	117	114	111	107	105	103	100	96
0.30													
A	333	278	226	204	191	182	175	168	160	156	151	145	139
B	167	154	142	135	131	129	126	124	121	120	118	116	113
0.25													
A	333	286	241	222	210	203	197	191	184	180	176	171	165
B	167	158	149	144	141	139	138	136	134	133	132	130	128
0.20													
A	333	294	257	241	231	225	220	215	209	206	202	198	197
B	167	161	155	152	150	149	147	146	145	144	143	142	141
0.15													
A	333	303	274	261	254	249	245	241	237	234	231	228	224
B	167	163	160	158	157	156	155	155	154	154	153	152	152
(*b*) PARABOLIC HAUNCHES													
0.5													
A	333	273	213	186	170	158	150	141	130	123	116	108	102
B	167	150	131	122	115	110	107	102	97	94	91	86	80
0.4													
A	333	283	233	210	196	187	180	172	162	156	151	143	134
B	167	156	143	136	132	128	126	123	120	117	115	112	107
0.35													
A	333	289	243	223	210	202	195	188	179	175	169	162	154
B	167	158	148	143	139	136	135	132	129	128	126	123	120
0.30													
A	333	295	254	236	225	218	212	205	198	193	188	183	175
B	167	160	152	148	146	144	142	140	139	137	135	134	131
0.25													
A	333	300	266	250	241	234	229	224	217	213	208	204	198
B	167	162	156	154	152	150	149	148	147	146	144	143	141
0.20													
A	333	306	278	265	257	252	248	243	237	234	230	227	221
B	167	164	160	158	157	156	155	154	153	153	152	151	150
0.15													
A	333	312	290	281	274	270	267	264	259	257	254	250	246
B	167	165	163	161	161	160	160	159	159	158	158	157	157

* n = the ratio of the length of the fillet, a, to the span length, L. † m = the ratio of the moment of inertia, I, at the center of the span, to the moment of inertia, I_s, at the support.

refers. Mr. Hall has contributed the very brilliant and useful suggestion that the deflected curve for the effect of a unit end moment is an influence line for end angle change. The writer would add that the angle change caused by the unit moment is an inverse measure of stiffness from which distribution factors are obtained and that the ratio of the angle change at the far end to the angle change at the near end is the carry-over factor for angle distribution. Hence, one loading (applied unit end moment) and a single analysis will give rise to all data for balancing angle changes.

It is suggested that the haunched beam is best analyzed for end slopes and elastic deflections by area moments preferably modified by the conjugate-beam conception. Table 3 gives the end angle changes for a wide range of haunched beams.

Mr. Mensch states that the effect of side lurch was overlooked in Fig. 2 of the paper. The fact that side lurch was not discussed was not an oversight but was premeditated and to this end Support F was shown by the standard convention of a non-movable support. If lateral movement had been permitted, the support, F, would have been shown by the standard convention for a rocker or roller nest. These conventions do not ordinarily receive explanation in a technical paper. Hence, it seems that Mr. Mensch erred in making his statement that "this example is a flagrant disregard of the fundamental conditions of equilibrium" about as seriously as he overstated the writer's actual claims when he wrote "the author claims that he has found a new and revolutionary short-cut in the analysis of indeterminate structures." These very modest "claims" are stated in the concluding paragraph of the paper as; (1) The method is self-checking; and (2) it offers the clearest possible picture of the physical action of the structure. The discussion of Messrs. Livesay and Simpson shows how the introduction of a roller nest at Support F would change the calculations.

Mr. Benson, Mr. Andersen, Professor Maney, and others have generously emphasized the self-checking features of the method. That the method is definitely self-checking is its greatest single advantage over methods that deal with moments or stresses alone. Others have mentioned the fact that the method gives a clear physical picture of the action of the structure corresponding with the various mathematical steps in the analytical process. Some would conclude, apparently, that this same advantage exists in all slope and deflection analyses, but this is most decidedly not true. No one would attempt to visualize any connection between the elastic action of the structure and the steps performed in solving a group of simultaneous equations either directly or by successive routine corrections. In decided contrast, it will be noted that every step in the process of balancing angle changes has a physical significance that is easily visualized.

Professor Maney and Mr. Gray seem to have misunderstood the use of the word, "plastic." The writer's use of the word was consistently that denoting "inelastic." The profession will make no mistake that elasticity means straight-line variation of stress with strain whereas plastic or inelastic deformation denotes permanent or non-recoverable deformation. Such a fantastic idea as "time yield" or "continued flow" in a structural steel joint which would practically eliminate end restraint is a proved fallacy that formed no part in the choice of the word, "plastic." This fallacy formed the design basis for the steel building frames of two decades ago.

In closing this discussion, the writer wishes to express his appreciation of the time and effort which the discussers gave to the clarification of the subject matter of the paper. This also seems an appropriate time to

express the conviction that the expanding group of modern methods of analysis based upon successive corrections which emphasize physical action under load will soon complete the displacement of the long entrenched classical methods dependent upon the solution of groups of simultaneous equations.

The following corrections will be made before the paper is printed in *Transactions*: In the discussion by Mr. Mensch, (March, 1937, *Proceedings*), for Equation (25*b*) write:

$$\text{“}\alpha_A = \frac{M'_A}{E\,K'_A}\,\frac{1 + \frac{4}{3}\,N_C}{4\,(1 + N_C)} \quad \ldots\ldots\ldots\ldots\ldots \quad (25b)\text{”}$$

In the line preceding Equation (30), change $\frac{\text{“}4\,(1 + 3\,N_C)}{(1 + 4\,N_C)\text{”}}$ to read $\frac{\text{“}4\,(1 + N_C)}{1 + \frac{4}{3}N_C\text{”}}$; in Equation (30 for “ $2 + 6\,N_c$” write “$2 + 2\,N_c$;” and, on page 583, change Lines 4 to 9, inclusive, to read:

“From Equation (25*b*),

$$m = \frac{4\,(1 + N_C)}{1 + \frac{4}{3}\,N_C} = \frac{4\,(1 + 0.278)}{1 + \frac{4}{3} \times 0.278} = 3.73$$

“From Equation (30),

$$M_{DF} = \frac{20\,000}{2 + 2 \times 0.278} = 7\,830 \text{ ft-lb}$$

which is a positive contribution to the moment in *DF* at *D*, producing there a positive moment of 7 830 − 4 930 = 2 900 ft-lb; which must be distributed * * *.”

AMERICAN SOCIETY OF CIVIL ENGINEERS

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DISCUSSIONS

THE MODERN EXPRESS HIGHWAY

Discussion

BY MESSRS. R. A. MOYER, AND CHARLES M. NOBLE

R. A. MOYER,[43] Assoc. M. Am. Soc. C. E. (by letter),[43a]—Many controversial questions are involved in the proposals offered by the author; however, the most important question that remains unanswered is that of deciding upon the proper speed or speeds for which the highways of the future are to be designed and for which existing highways are to be re-designed and improved.

The writer discussed this subject at some length in 1936 and 1937,[44] including a recital of the more general aspects of the problems involved. In analyzing Mr. Noble's paper, the writer wishes to add further interpretations of the results of his extensive research on the subject of speed *versus* safety, as it applies more specifically in this case to the problem of designing express highways for 100 miles per hr recommended by Mr. Noble as a suitable design speed.

There seems to be general agreement among highway engineers to-day that many highways in the United States are in immediate need of modernization and that the greatest single factor which has been responsible for this situation is speed. Highway engineers are by no means in agreement in regard to the methods to be adopted to solve the many problems created by wide variations in speed and especially by travel at high rates of speed. The author leaves the impression that the best solution to the speed and highway accident problem lies in the design and construction of express highways for speeds of 100 miles per hr. After more than five years of intensive study of the many factors related to speed, the writer is convinced that safety on highways can not possibly be assured

NOTE.—The paper by Charles M. Noble, Assoc. M. Am. Soc. C. E., was published in September, 1936, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: November, 1936, by Messrs. Fred Lavis, Joseph Barnett, G. E. Hawthorn, John F. Fairchild, Leslie R. Schureman, and C. H. Purcell; December, 1936, by Messrs. Elmer R. Haile, Jr., H. W. Giffin, and T. T. Wiley; January, 1937, by Messrs. F. L. McRee, Theron M. Ripley, W. W. Crosby, Richard S. Kirby, Harold M. Lewis, George Conrad Diehl, and William E. Rudolph; February, 1937, by Messrs. A. C. Dennis, and J. C. Carpenter; and March, 1937, by Messrs. Henry B. Alvord, Robert Eugene Hiles, and Chandler Davis.

[43] Assoc. Prof. of Highway Eng., Iowa State Coll., Ames, Iowa.

[43a] Received by the Secretary April 2, 1937.

[44] "Speed *versus* Safety on Straightaways", *Civil Engineering*, November, 1936; and "Speed *versus* Safety on Curves", *Civil Engineering*, February, 1937.

by following such a course and that the surest way to increase accidents and fatalities is to encourage high-speed travel by building highways permitting speeds of 100 miles per hr. Not only is travel at such speeds unsafe on the basis of the physical laws which govern motor-vehicle operation and which are inviolate, but the difficulties which confront the driver at such speeds constitute an even greater threat to his safety. Furthermore, the extra costs of providing suitable highways for travel at this speed, and especially the excessively high operating costs, definitely indicate that such travel is uneconomical and impractical.

The author may defend his stand on the basis that if the highway is designed for a speed of 100 miles per hr, it will be safe for speeds less than that. Now, it is the writer's opinion, that this attitude is contrary to the first fundamental concept of good design offered by the author when he states that "it is possible to design a highway so that the motorist is led unconsciously to choose the safe act rather than that which is unsafe," and he further points out "the desirability of designing the highway on the basis of a constant rate of vehicle speed between large terminal points." With these concepts or principles of design the writer concurs. However, few persons driving cars at 100 miles per hr on a highway of the type suggested by the author could ever be expected to have the ease of mind or feeling of safeness commonly experienced when driving at the moderate speed of 50 miles per hr on a dual highway or even on a two-lane highway designed to present-day standards to accommodate such a speed. The tenseness and feeling of unsafeness commonly experienced at speeds greater than 60 or 70 miles per hr on the best of present-day highways is born out of the knowledge that there is a certain minimum time limit to any driver's reactions to meet certain emergencies and errors of judgment below which it is impossible to go. In that split second so much can happen over which the driver has no control that only by taking the control of the car almost completely out of his hands (as has been done in the safest rail operations) can safe operation be made possible. Not only is there a fixed limit to reaction time but, what is more important, the forces with which the driver has to contend at a speed of 100 miles per hr have been increased many times, compared to the forces encountered at present-day average speeds of 40 to 50 miles per hr. The time required to bring the car under control has likewise been increased; and the distance traveled during this interval extends far beyond the zone of safety within which control of the car may reasonably be assumed to be possible. Only a revolutionary change in the steering, braking, and driving of cars can correct this situation. Accordingly, even if the driver were led unconsciously to choose the safe act on a highway designed for 100 miles per hr, his chances of following his choice when driving at this speed would all too frequently be very small, indeed, to prevent the accident.

A valuable feature of this paper is that it has directed attention to the most vital factor in modern highway design—the speed factor—and

furthermore, it has stimulated discussions on definite design details which the author contends should provide positive safety at a definite speed. However, to establish the design speed arbitrarily at 100 miles per hr, or at the maximum speed at which the cars are now, or may soon be, capable of, is contrary to another basic principle in design mentioned by the author in which he states that to-day emphasis should be based on broad highway economics founded on loss of life and property as well as on operating costs.

In the 1936 report of the National Safety Council on "Accident Facts," statistics are given which indicate that the severity of accidents increases with an increase in speed. Thus, at speeds of less than 20 miles per hr, 1 accident out of 61 is fatal; at speeds between 20 and 29 miles per hr, 1 accident out of 42 is fatal; at speeds between 30 and 39 miles per hr, 1 accident out of 35 is fatal; at speeds between 40 and 49 miles per hr 1 accident out of 25 is fatal; and at speeds greater than 50 miles per hr, 1 accident in 11 is fatal. Statistics of railroad-crossing accidents, involving cars and trains, have consistently shown that one accident in two or three is fatal. Tremendous forces are involved in accidents involving high fatality rates. It should be noted that the forces developed at high rates of speed vary as the square of the speed and that the aforementioned statistics indicate a certain amount of correlation in this respect.

Not only is the severity of accidents increased with an increase in speed, but the chances for accidents increase with an increase in speed. This was revealed in 1936 in a report,[45] by Charles J. Tilden, M. Am. Soc. C. E., in which the accident records of owners of cars observed traveling at high and low speeds were analyzed. It was found that in this instance "the high-speed drivers who have accidents have more of them, so that they account for 45 per cent more accidents than do the low speed drivers."

In regard to the economics of express highways, Mr. R. E. Toms summarized the highway cost phase of it very ably in a paper presented at the 1937 Annual Meeting of the American Association of State Highway Officials.[46] He stated that,

"* * * however desirable they [express highways] may be, they are not possible except on a very limited mileage of our State Highway Systems. * * * Assuming that it would be desirable to improve 5 per cent [about 16 000 miles] of the State highway mileage with four or more traffic lanes with opposing traffic separated, grades at intersecting highways separated, border roads to eliminate unrestricted access from abutting property, and sidewalks for pedestrians where needed, the expense involved in this undertaking alone would amount to approximately four billion dollars. This is just about the amount that has been devoted to highway improvement in this country during the last 20 years through the program of Federal aid to the States matched with State funds, plus the amounts of Federal funds appropriated for emergency construction which did not require matching by the States. When these figures are considered,

[45] "Motor Vehicle Speeds on Connecticut Highways", pub. by Committee on Transportation, Yale Univ., 1936.

[46] "Highway Safety Exemplified by Properly Designed and Constructed Highways", by R. E. Toms, *American Highways*, January, 1937.

we must admit that in so far as we can visualize the future at this time from 95 to 97 per cent of the State highway mileage in this country may never progress in improvement beyond a two-lane highway."

Although Mr. Tom's estimate of the cost of constructing express highways may have been high, since it calls for an average initial cost of about $250 000 per mile, this figure is not unreasonable when stated in terms of the cost per vehicle-mile on a highway carrying 4 000 or more vehicles per day. The annual cost for such a highway, including maintenance costs, snow removal, depreciation, interest charges, and charges for policing, would probably be about $15 000 per mile and, for 4 000 vehicles per day, this would represent a tax cost of about 1 ct per vehicle-mile. The present average tax cost for highways in the United States, based on the license fee and gas tax, is about $\frac{1}{3}$ ct to $\frac{1}{2}$ ct per vehicle-mile. It would be quite possible for the Federal Government to undertake such a project and to assume full control of this system of highways. A Federal license would be required to operate on the highway. Tolls might be collected to finance the cost of construction and operation. The highway would be constructed, maintained, operated, and policed by the Federal Government.

Although the highway cost may not be unreasonably high where the traffic volume is sufficiently great to justify the construction of such a highway, the motor-vehicle operating costs are so much higher at 100 miles per hr than at the present-day average speeds of 40 and 50 miles per hr, as to make it very doubtful whether traffic would operate at such speeds for any considerable distance; and, certainly, it would be absurd to build highways for such a speed if only a small percentage of the traffic would travel at the designed speed, or if drivers were to operate at such a speed for only a few minutes at a time. Tests conducted by the writer indicate that fuel costs at 70 miles per hr are almost double the costs at 40 miles per hr, and at 100 miles per hr there is every indication that the costs for the same car are about three times the costs at 40 miles per hr. Oil costs seem to follow a similar trend, or if oil costs are reduced at the higher speeds, the engine costs are increased, due to increased wear on cylinders and pistons. Tire wear follows the square law very closely and the tire costs at 100 miles per hr are most likely to be about six times as great as at 40 miles per hr. Engine breakdowns are still very common in the Indianapolis, Ind., races where an average speed of slightly more than 100 miles per hr is maintained for only 500 miles. One can be quite certain that engine repair costs and car maintenance costs would be greater at 100 miles per hr than at 40 miles per hr. A conservative estimate of the increased operating costs for the same car due to speed is that the cost of operating at 70 miles per hr is 2 cts per mile more than at 40 miles per hr and at 100 miles per hr it may easily be 4 or 5 cts per mile more than at 40 miles per hr. Such a cost of high-speed travel can not be disregarded when proposals are made for spending billions of dollars to build express highways. It would be a sad commentary on engineering judgment to find that after

such highways were built, traffic would not be willing to pay the tax to use them—the toll-free State highways with their lower speed limits carrying most of the through traffic.

In his discussion of details of design, the author states that there is great need for research before satisfactory design details can be developed. It has been gratifying to note that this paper has provided the means for bringing together many discussions in which considerable experimental data and many excellent theoretical analyses of the design problems are presented for the first time. Tests conducted by the writer[47] have provided much basic information that is needed in the solution of this problem. Although present knowledge is far from complete, it is the writer's conviction that the basic facts are now known, and it is not so much additional information that is needed as it is the proper use and interpretation of the information at hand.

The most important element in the design of an important highway to-day is the speed factor. This is not a matter to be decided upon by an individual, from experience based on local conditions, but rather it is an element of design which should be as rigidly controlled as are the weights, lengths, widths, and heights of vehicles. Speed may quite properly be thought of as a "fourth dimension" which controls curvature, superelevation, road width, and sight distance. In fact, practically every detail of design is wholly or in part controlled by the speed factor. For important highways, it is desirable, therefore, to standardize or control speed for the same reason that weights and lengths of vehicles are standardized. The establishment of such a standard is not an easy matter.

In this discussion, the writer has touched on only a few of the more important factors which should be considered before speed standards are decided upon. A complete analysis of the speed problem and of establishing design speed standards for various classes of highways is not within the scope of this discussion.

It is virtually impossible to design highways to permit safe travel on ice-covered surfaces for the high-speed travel, which the author suggests will be a controlling factor in determining the superelevation on curves. Tests clearly indicate that there simply is not sufficient friction available between rubber tires and ice to make it possible to develop a speed of 100 miles per hr on a level surface, not to mention the difficulties of steering or of stopping the car at that speed. The writer recommends that superelevation be used on all curves to permit easy steering and that the maximum rate of superelevation of 0.1 ft per ft be used on the sharpest curves, combined with a maximum value of f equal to 0.1 where this frictional force is necessary to counteract centrifugal force not taken care of by the superelevation. The safe speed on ice is almost never greater than 30 miles per hr on straightaways where traffic is separated and it certainly does not seem possible that curves could be designed to permit higher speeds with safety on ice by any known device

[47] *Bulletin 120*, Iowa Eng. Experiment Station, Ames, Iowa.

commonly used in highway design to-day. The writer wishes also to state most emphatically that spiral curves are necessary and may very properly be used on all curves.

In this paper, the author has aroused the imagination of his readers. The paper has been timely because this country now appears to be approaching the end of what has been aptly called a "highway construction holiday." A new era of road building seems to lie immediately ahead. The modern car is so far ahead of the highways on which it must now travel that mere improvising will not suffice. A new conception of a modern highway is needed. Men of vision and of wisdom are needed, who can weigh all the variables involved, who can foresee the direction in which true progress lies, and who can formulate the broad plans upon which the highway systems of the future are to be built. The magnitude of the improvements under consideration are worthy of the most thorough and painstaking analysis and it is for this reason that this paper should prove to be a really worthwhile contribution to the literature on the subject and a benefit to all the people in this country who, in the last analysis, will profit most by such studies.

Charles M. Noble,[48] Assoc. M. Am. Soc. C. E. (by letter).[48a]—The response to the paper, and the frank expression of opinion, is gratifying. The discussion has been directed as much along sociological, psychological, and philosophical lines as along a train of thought devoted to technical design. Of the three elements which constitute the broad background of highway safety (namely, the operator and his control, the vehicle, and the highway) the paper treated only the latter, on the theory that the other two were, in a technical sense, out of the province of the designing engineer; but, since the discussion has broken free of the original limits, it may be in order to continue in the spirit of the discussion and begin at once with the non-technical phases before turning to the more technical features.

The writer wishes at once to concur with all that has been said respecting the desirability and advantage to be gained from a rigid restriction of the privilege of driving a motor car on the public streets and highways, as well as the adequate testing and training of the operator. There is no doubt that a very large percentage of accidents can be prevented in this manner.

The deprivation of one's license to drive, from the reckless and unfit, would relieve the highway at once of one of the gravest hazards, while at the same time it would have a sobering effect on the remaining drivers. Nothing in the paper could be construed as advocating looseness or laxness in this direction and, for some years, the writer has been keenly aware of the advisability of rigid control and policing. However, such control is more in the province of the administrator, the police official, the politician,

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[48a] Received by the Secretary March 25, 1937.

and the engineer, acting in the broad capacity of his profession. Essentially the problem of the designing engineer is to design to comply with conditions as he finds them, or as he can reasonably predict they will be in the future. Perhaps a short review of elementary basic engineering design principles may not be out of order, as there appears to be a tendency to overlook some of the fundamentals which general engineering practice has evolved.

In designing a bridge, for example, the first task of the designer is to discover the magnitude of the loads which the bridge may be required to support, the physical properties of the materials of construction, the proper factor of safety to apply to the materials, the determination of the stresses of all kinds which will occur in all parts of the structure, and, finally, the proper proportioning of each member, based on the transmission of stresses to it. The completed design represents a logical and consistent process of thought, which combines a close attention to detail with a clear picture of the functioning of the structure as a whole. It is axiomatic that success in engineering design is dependent on the correct assumption of the loads to be carried, the proper selection of the materials of construction, and the correct stress to apply to these materials.

Suppose bridge engineers designed structures, using loads less than the actual known loads, because they felt the actual loads were unreasonably high, that loads of such magnitude should be prohibited, and because they hoped legislation might be passed some time in the future to curtail the loading! This seems to be somewhat the attitude of a number of highway designers. There appears to be a reluctance to acknowledge the loads which the highway is called upon to support.

Traffic density and speed are the loads. Alignment, grades, superelevation, sight distance, shoulders, signs, lighting, and a multitude of details which add to vehicle safety, are the materials. Loads have been assumed that were too small, and the materials have been stressed up to and beyond the ultimate strength. This is not in accordance with established engineering practice.

A brief review of recent highway engineering history may be of assistance in clarifying past design measures and in illustrating that design practice is, at present, pausing at the cross-roads, while consideration is being given to the direction it shall pursue. The twenty years since about 1917 represent the pioneering period in highway design as related to the motor car. At the beginning of the period no road system, as at present defined, was in existence, and the motor industry was still in its infancy. The pressing need was to bring into existence, from a standing start, a connected road system embracing the entire United States. The task was colossal. Cheapness of original cost and speed in location and construction were the goal.

At that time, the property owner contributed the bulk of the road money, and he expected a road dollar to go a long way, geographically. The results may be compared in a rough sense with the earlier development

of the railways, which, although not of a thoroughly permanent nature, were, nevertheless of vast benefit to the nation. America was "pulled out of the mud" in the incredibly short time of twenty years, thus making possible the almost magical development of the motor car and, even if the exigencies of the period had permitted of more leisurely and permanent construction, engineers had nothing on which to base a forecast of future requirements, for no one really dreamed of a stock car of moderate price which could travel with ease hour after hour at 70 or 80 miles per hr. Engineers were literally pioneering in a new form of transportation, and, consequently, one must not be too critical of their performance.

To-day, the situation is quite different. The pioneering period has definitely closed, and the highway and motor industries are entering upon an era of early maturity. The motor car owner is contributing the major portion of the monies for highway improvement, and consequently his safety is entitled to careful consideration; in addition, there is ample "case history" from which to make an intelligent forecast of future needs. From now on the engineer must produce a finished and polished product, perfected as to detail and equipment, and, in addition to expeditious travel service, an adequate margin of operating safety must be provided.

There has been a misconception on the part of many readers. It has been assumed that the writer is a proponent of speed, and that he advocates an improvement in design for the purpose of providing speedways to encourage the motorist to drive at high speeds. Nothing could have been further from the writer's mind. He does feel, however, that a great number of motorists always have driven at a higher rate of speed than road conditions warranted, and that they will continue to do so until road conditions are improved to a state more in accordance with the speed habits of the nation. As far as the writer personally, is concerned, he would be content never to exceed 50 miles per hr, but the problem the engineer is facing cannot be solved by injecting personal opinion into design practice. Engineers are designing an engineering structure, and engineering principles and methods must be applied—principles and methods which have evolved from the design of other engineering structures.

There has been considerable discussion and adverse criticism by many commentators respecting the writer's suggestion that design be based on a speed of 100 miles per hr. It must be recognized at once, however, that a design for express routes, based on this speed, is a very effective method of assuring a proper factor of operating safety for more moderate speeds, while at the same time providing a reasonably sure form of insurance against obsolescence. As subsequently demontrated the adoption of such a design speed does not involve as much difference in actual design standards as might be supposed in comparison with a design based on a sustained speed of 60 miles per hr. Be this as it may, the embarrassing question may quite reasonably be asked: Is there anything in the past

history of the development of the motor car which justifies a confidence that top driving speeds have been reached? It should be remembered that the motor industry is highly competitive and therefore, will continue, in all probability, to strive to give the public the kind of vehicle which it demands.

As far as the speed situation is concerned, present design practice appears to be in a somewhat unfortunate position. If the road was so crooked or so congested that even moderate speed was physically out of the question, it would be reasonably safe as far as serious accidents are concerned; or, if the road was designed uniformly for a speed greater than any one cared to drive, then it would be safe from a physical standpoint at all the operating speeds to which it might be subjected. Present design, however, is neither the one nor the other. It is open enough to encourage speeding, but not open enough to justify such speeding, and thus highway designers are guilty of encouraging the motorist to operate at speeds not justified by the actual design. It should be emphasized that safe design consists not only of good alignment, super-elevation, and grades, but upon a skillful combination of all the components outlined in the paper, so that a consistent and ordered whole is produced. The writer earnestly feels that engineers should face the facts squarely, should make an intelligent effort to forecast the future, and should then proceed in an ordered and logical manner to design along engineering principles, adopting a proper factor of safety.

The key to the widespread advance in sustained road speeds appears to be related more to the general improvement of the vehicle rather than specifically to increase horse-power in the motor. As long ago as 1920 there were a number of makes of American stock cars which were capable of speeds of 75 and 80 miles per hr; but, except for the most reckless and adventurous, there were few that even approached these speeds, because driving one of those cars at such high speeds was distinctly a nerve-wracking experience which only a thrill seeker would endure. The human nervous system could not stand continuous high-speed operation in such cars, and, consequently, continuous operation seldom exceeded 40 miles per hr. Thus, highway engineers had no experience or any other basis for predicting a rapid advance in road speeds. Along in the middle 1920's, when the manufacturers had mastered the production of a fully dependable motor, they turned their attention to other features of the vehicle. Improvements in the body, steering, tires, brakes, reduction in vibration, redistribution of weight, shock absorbers, rubber mountings, etc., pyramided in rapid succession, until to-day the car glides over the highway with swiftness and ease, eradicating all sense of speed values, with complete lack of nerve tension and fatigue for the operator.

In a word, all sense of speed has been destroyed. This, then, is the major cause of the general increase in sustained road speeds, and coupled with greater power plant in the medium priced and cheapest vehicles, it spreads the power of high speed among the great mass, and among all

classes of people. The destruction of the sense of speed is the primary reason why many conservative motorists operate at higher speeds than conditions warrant. This is particularly true where conditions require speeds in the lower brackets.

A recent advertisement of a car "priced just above the lowest" listed, among others, the following: "No vibration or road rumble; new ease of steering; no 'wind wander' even in a gale; no 'edging off' in ruts or gravel; new sound-proofing; so silent inside you talk in a whisper; 'city ride' on any kind of road; * * *" Incidentally, this car is equipped with 93 hp. There can be little doubt that manufacturers will soon reduce the cost of high speed. In view of the highly competitive nature of the industry, do highway engineers really believe advancement in automotive design will cease abruptly?

Mr. Purcell gives the results of observations in California showing that 94% of all cars traveled at or less than 55 miles per hr. Assuming the observations were taken in 1936, it should be remembered that more than 60% of these cars were three years old, or older. In other words, 60% were not newer than the 1933 model, and many of the most effective improvements in vehicle design which particularly deaden the sense of speed, have occurred since that year.

Since comparisons are inevitable, suppose for a moment, the essential difference between improvement in railway line and improvement in motor ways is examined. The improvement in track is entirely economic, using the definition in the conventional sense. Such improvement is actuated by a desire to lower the cost of hauling freight or to shorten passenger-train time. Safety is not an issue, because enginemen are given a definite speed order to apply to each section of track, and they are perfectly familiar with their runs. Because these operators are employees who may be disciplined for infraction of orders, obedience is quite general. Thus, from a safety standpoint, bad track conditions can be overcome.

The highway situation is quite different. The motorist, by and large, is not an employee, and he purchases a vehicle from an industry which is not responsible for the design, construction, or maintenance of the track upon which it operates. As long as he can escape the vigilance of law enforcement officers he can do, and does, about as he pleases. He usually feels he is clever if he continues to "get away" with breaking the motor laws. Often he is unfamiliar with road conditions, and more often is operating at a greater speed than conditions require. He has no "slow orders," and, therefore, any sudden change in road conditions which requires a slow speed introduces a hazard which, sooner or later, results in an accident. Moreover, the average motorist does not have the skill and judgment which an engineman possesses. Thus, an improvement in highway standards is a step forward in safety, and consistency in design is the very essence of safe design.

What, then, is there in this situation to assure the highway designer that he will ever catch up with automotive design; that obsolescence will

not continue its costly toll? As far as the vehicle is concerned, there appears to be no immediate limit; possibly fantastic speeds may be possible, but a limit there must be, and that limit is more likely to be human flesh than steel and rubber. The human nervous system has a limit, and it has been stated that this limit is possibly 100 miles per hr. This, then, is the basis for the suggestion in the paper that 100 miles per hr may not be an unreasonable maximum design speed.

The purpose of the paper was to show that the present standard of first-class road design is inadequate for high speeds. The destructive forces at high speed are so terrific that an entirely new conception of trunk highways will be necessary to cope with the situation. This thought is expressed very aptly by Professor Moyer. The writer is not sure, given unlimited resources and money, whether engineers could produce, to-day, an inherently safe design. They may not know enough at this moment about safe design. Nevertheless, a sincere and intelligent effort must be made to provide the motorist with as much protection as knowledge and skill will permit, at the same time continuing an intensive attack on the problem in order that the profession may the more surely advance its knowledge and technique.

In general, the discussion by the commentators may be classified into two more or less distinct groups: (1) That the present standards of design are properly and efficiently performing their functions, and that the highway system, as at present constituted, is as perfect as the motorist may reasonably expect; and (2) that further advancement in the art of highway engineering, particularly as related to safety of operation, is distinctly indicated if the motorist is to be provided with the type of service he should reasonably expect.

It may be well to express to proponents in the first group that no engineering art should remain stationary—that none of the various branches of engineering is remaining stationary When an art remains stationary it becomes mere dogma. The writer feels sure no one really wishes to suggest such a fate for highway engineering. Indeed, it is clear that this branch of engineering has just emerged from the pioneering period and there is a vista ahead of mature but vigorous growth and development.

Specifically, the following suggestions have been offered in lieu of improved design standards, or as reasons against raising such standards:

(1) That vigorous control of the driver should be maintained and the unfit and the accident repeater be ruthlessly ruled off the road;

(2) That vehicle speed be arbitrarily controlled by a mechanical device;

(3) That a large class should not be taxed to provide speedways for the few;

(4) That the cost of adequate highways is too large and is not economically justified, and that the percentage of accidents caused by faulty highways is not great enough to justify an improvement in present standards; and,

(5) That it is questionable whether it is the duty of the State to provide expeditious service.

It may "clear the air" somewhat if Suggestions (1) and (5) are discussed in somewhat greater detail.

Control of Driver.—For twenty years the problem of control (Item (1)) has been discussed but very little has been accomplished. A few years ago it was thought that a system of licensing drivers would be a remedy, but such action has been found to be only mildly palliative. The difficulty is that the great mass of public opinion is opposed to rigid control measures; politicians will not pass statutes, nor will officials ruthlessly enforce legislation, which they know is not solidly supported by the mass of the people. The Eighteenth Amendment is an example.

As a group, highway engineers would welcome the elimination of all unfit and dangerous drivers, particularly those who prove to be accident repeaters. Granted such a procedure is most desirable and that it would be helpful, it would by no means solve the problem under discussion. It would not materially reduce those accidents caused by imperfections in the highway itself. Many careful and competent drivers have serious accidents. Should sober and substantial citizens be required to risk their lives day by day when it is possible to remove many of the hazards connected with motoring by designing safety into the highway?

Unfortunately, a continued discussion on the subject of control is almost academic. Does any one really believe that rigid and drastic laws will be passed and enforced in the near future? Without wishing to appear cynical, the writer believes the answer is definitely, "No!" Only by the slow process of education can real progress be made, and for this reason it is doubtful whether the goal can be reached within the next twenty years. By that time nearly 800 000 human beings will have paid with their lives, if the present death rate continues, and 25 000 000 will have been injured.

Mechanical Control Devices.—During the past twenty years mechanical speed governors (Item (2)) have been seriously proposed several times; but the public would have none of it. It is one thing for a private company operating a number of motor vehicles to apply governors to their cars but quite another to get a law on the statute books requiring all citizens to apply governors. An employee of a company is quite helpless to prevent the use of such a device, but the electorate have a way of showing their disapproval. In addition, there are practical difficulties.

Would such a law be a Federal statute, or would each of the forty-eight States have to pass legislation specifying an identical speed limit? Would motorists from other States be compelled to stop at State lines and purchase a governor in order to pass through States having such laws? The injection of these conditions into the motor problem would undoubtedly disrupt motor transportation. Finally, is there any one who can point to a strong, unified movement among the great mass of citizens for an enactment of mechanical speed legislation?

Taxation.—The writer can recall hearing, twenty years ago, the same complaint from owners of horses and buggies to the effect that the taxpayers were having to spend good money to create speedways for the motorists (see Item (3)), but does not recall hearing any motorists in recent years complaining about roads being constructed too well. Twenty years ago most of the road funds were being contributed by the property owner. To-day, most of the road money is being contributed by the highway user. As Mr. Dennis states, the American highway is, in effect, a toll road, the user paying a sales tax. For instance, in 1935, the State and Federal oil and gas taxes and license fees amounted to $958 753 000, the manufacturers' and personal property tax to $350 000 000, making a total of $1 308 753 000. If the motorist, who pays the bills, does not object to the expenditure of sufficient money to modernize the highways, there is no valid reason why the engineer should urge him to accept an article which does not provide him with reasonable safety facilities. To the writer's knowledge, there is no unified and concerted movement among the various associations of motorists for the curtailment of highway construction.

Economical Considerations.—It is often argued (see Item (4)) that it costs too much money to do a first-class job in constructing highways. That may have been true during the pioneering period when the vital need was to create a road system; but now that that era of stop-gap construction is past, engineers can proceed to replace their "log cabins" with brick and stone. It must be realized that adequate highways will cost a deal of money, and it would seem the better policy to design and construct these highways with the thought of their being permanent, rather than to have to repeat the performance twenty years hence, thus wasting the sums now being spent. A cheap article is not always a bargain.

It is somewhat inexplicable that engineers should take the stand that adequate highways should not be designed and constructed because of the large cost involved, when it is remembered that considerable sums are diverted annually from motor vehicle funds to other than highway uses. A more commendable effort would appear to be to educate the public to the fact that the need of highway expenditure is not over, but in reality only begun, and that the highway industry needs every dollar collected from the motorist to remove hazards from the present highway system and to construct new highways in accordance with advanced technique, to the end that the public will receive more expeditious road service, will derive more pleasure from motoring, and will not be subjected to as many chances of being killed or injured. Thus, the engineer would be performing a notable public service, at the same time assuring himself, and his fellow engineers, of a continued opportunity to make use of his technical talents.

It is further contended that the percentage of accidents caused by faulty highway design is not sufficient to justify the expense of safe design. This is quite a cold-blooded argument when the terrific mental and physical

suffering occasioned by traffic accidents is remembered. To make matters worse, the argument may not be altogether true. As stated in the paper, accidents occasion an economic loss from a purely financial point of view. The writer is not aware of any accurate appraisal of this loss. The Travelers Insurance Company, of Hartford, Conn., estimates that it amounts to from \$2 000 000 000 to \$2 500 000 000 each year. W. V. Buck, M. Am. Soc. C. E., states[49] that statistics indicate that 30% of all accidents may be traced to some fault in road location and design. If these figures and assumptions are correct, then faulty highway design is costing the American public from \$600 000 000 to \$750 000 000 each year; and to these figures must be added the economic loss due to obsolescence. It is evident that more study is necessary in order that more nearly accurate figures of the economic loss due to accidents may be made available.

In this connection it may be pertinent to mention railway-highway grade-crossing elimination. Acording to Table 1, only 0.6% of all accidents were due to this cause. If the economic loss from this type of accident is assumed to be at the same rate as for all accidents, then, assuming the foregoing estimate, the annual economic loss due to railway grade crossings only amounts to from \$12 000 000 to \$15 000 000 for the entire United States. Yet, as Mr. Ripley states, New York State recently voted \$300 000 000 for grade-crossing elimination, and there is a strong movement all over the country for rapid elimination of all railway grade crossings.

Studied from a cold-blooded standpoint, this work has no economic justification; but is there any considerable body of engineers who condemn grade-crossing elimination? If engineers are willing to support such work, why are they not willing to support the elimination of other road hazards? Mainly, it is because of the subtlety of the problem. The actual causes of other types of accidents are not as evident even to engineers.

The public is supporting grade-crossing elimination heavily because it can readily visualize a railway crossing as a hazard. If the public really understood the mortality due to other highway hazards, they would without doubt approve the expenditure of the necessary funds to provide for their systematic elimination. More people are killed in motor accidents in the United States in one month than have perished in all the floods in the entire country during the past ten years. Therefore, it would appear wise for engineers to advise the public of the relation between highway design and the accident rate, rather than to remain indifferent until stung into action by public censure.

Responsibility.—Mr. Lavis questions whether it is the duty of the State to provide the motorist with expeditious road service (see Item (5)). This is a pertinent question and deserves careful consideration. As practiced in the United States, the State has, in general, assumed as a duty any function for which there is a persistent and widespread demand among the electorate. It may be assumed, therefore, that if the public demands

[49] *Engineering News-Record,* October 29, 1936, p. 612, *and Civil Engineering,* January, 1937, p. 1.

expeditious service, the State will accept the responsibility within the limits of available highway revenues. The continued reconstruction of outmoded highways is an expression of an acceptance of that duty. Frequently, the relief from congestion in urban areas constitutes a provision of expeditious service, and highway engineers are in a position to know to what lengths the State is willing to go to provide such service.

From the foregoing discussion, it is rather evident that designers are facing (and probably will continue to face for some time) operating conditions over which they have little control. Thus, an open mind, continued thought, compilation and analysis of data, and continued technical progress, based on well-founded principles, is clearly indicated in order that that happy combination of safe design and over-all economy may be achieved.

Rights of Way.—Of all the features under discussion, the alignment and width of the right of way is by far the most pressing and important, not only from a financial point of view, but also because of its vital influence on vehicle safety. The ability of the average citizen to look into and prepare for the future is generally conceded to be rather poor. His thoughts are chiefly concerned with the present, and usually he is not prepared to make any sacrifices or provide monies to care for the future unless he can be shown conclusively the benefits that will accrue. The dissemination of this gospel and education is in the province of the engineer, for he is generally thought to have the ability to look into the future, to forecast the coming needs, and to evaluate, properly, the extent to which the present generation should prepare for the problems of the next. The water supply, flood control, electrical, and sewerage engineer has demonstrated his proficiency in this field, and has educated the public to respect his judgment. The right-of-way situation presents an opportunity to the highway engineer for public service.

Messrs. Lavis and Crosby have indicated the desirability of initially acquiring ample rights of way with proper alignment. The importance of the subject justifies an expansion of the discussion in order to focus attention sharply upon this vital question.

The right of way is the only element of the highway that is permanent. This axiom was current when the writer entered the highway field in 1914, yet it was heeded to so small an extent that to-day the inadequacy of rights of way, which is the most difficult and expensive error to correct, is acute in many States. Road improvement brought an increase in land values and physical improvement adjacent to the right of way which has made widening and straightening prohibitive in many cases. The critical street situation in cities is a manifestation of the ultimate results of inadequate width. As stated previously, lack of vision during the pioneering stage in securing adequate rights of way should not be criticized without a realization of the problems of the time. To-day, the situation is very different.

Because of its very nature, the trunk highway, will be located to as large an extent as practicable upon new rights of way as far removed

from developed areas as possible, and, in general, it will be purchased by the acre rather than by the square foot. Therefore, its cost will be less at the time of original improvement than at any period in the future. It can be purchased on the basis of the owner retaining (with restrictions), the mineral, timber, and cultivation rights of the unused part, thus reserving to him a large portion of the material benefits for many years, while, at the same time, he would be relieved of paying taxes on such lands. This procedure will reduce the cost of acquisition, and, at the same time, lessen the opposition of the public and the land owner.

In the discussion by the commentators, frequent mention has been made of the excessive cost of wide rights of way. The cost of anything is relative. Bearing in mind that the life of such land is relatively infinite, the yearly cost is consequently very small; but, for fiscal reasons, it may be advisable to liquidate it within a period of 50 yr, or possible 100 yr. In Europe, there are rights of way in use to-day which have existed for more than 2 000 yr. The right of way is the very foundation of the pyramid of stage construction.

The initial construction (located off center), may cost less than the land, and in undeveloped regions its standards may be quite low (except for the alignment which is determined by the right of way), until traffic justifies further improvement, but the ultimate construction may cost $200 000 per mile. (In the East, highways have already been constructed through open, easy country which cost $600 000 per mile, exclusive of land.) If the right of way proves inadequate, the previous heavy investment in physical construction may be rendered impotent because when a highway is relocated to a new right of way, all improvements in the form of grading, sub-drainage, stabilization of sub-grade and slopes, drainage, bridges, and landscaping are lost. It is a type of financial failure. If the right of way is of proper alignment and of sufficient width, each step in advancing the standards will make full use of all previous effort and expenditure, thus conserving the people's investment and producing, step by step, a more perfect and up-to-date element of transportation.

It is difficult for those who reside in rural or undeveloped regions to visualize the throttling effect of growing population and development upon inadequate rights of way. Many troubles to-day are caused by the failure of previous generations to realize the possibilities of the future. It was difficult for them to visualize the effects of growth. Many wholly undeveloped regions through which right of way may be purchased at cheap prices, may suffer 100 yr from now if a niggardly right-of-way policy is pursued to-day.

An element of cost not sufficiently stressed is the adverse effect upon, and economic loss sustained by, farmers and property owners each time a highway is relocated. Many farms and parcels of property have been cut up into unusable or uneconomic pieces. Areas containing abandoned pavements usually are not reclaimed and put back into any form of production, and an abandoned right of way seldom finds its way back

on the tax books. Certainly highway authorities should not subject land owners to this form of inconvenience and loss more than is necessary. Permanent right of way, of adequate width, would solve the difficulty and tend to stabilize land values, particularly when the freeway principle is used.

The present period appears most opportune to advance the cause of wide rights of way, in view of the continued statements from high governmental sources relative to retiring large tracts of land on the theory that more land is in use than is economically justified, and highway engineers should be quick to seize the opportunity of informing the public of the lasting benefits which will accrue to the nation from an enlightened right-of-way policy.

Classification.—Mr. Diehl brings up the very pertinent question of highway classification. The subject is not only important but pressing. The road must not only be classified according to present and future traffic trends, but it is quite as vital for it to be classified on the basis of safe operating speed. Fortunately, the groundwork[60] on which to base a classification is being laid by forty States in co-operation with the U. S. Bureau of Public Roads. Whether full and proper use will be made of these transport surveys depends on the initiative and unselfish public spirit of officials and engineers. For the first time, it will be possible to make a real and searching analysis of the social and economic structure of the highway system as a whole.

National Highways.—Without doubt, when the local and regional analysis is completed, thinking will be directed toward the possibility of a national system of highways, designed, constructed and maintained by the Federal Government. This is indicated by Mr. Dennis, and such a system may not be as fantastic as would appear at first thought. Undoubtedly, it would prove a real national asset not only for the every-day needs of the nation, but also as a vital unit in the national defense system. As Mr. Dennis intimates, a national system may be likened to the trunk lines of the railways, with the present system acting as the branch or feeder lines.

If it is assumed that 10 000 miles would constitute a reasonably complete system, it could be constructed for approximately \$3 000 000 000, and if, as Mr Diehl suggests, it is developed within a period of 30 yr, the annual appropriation required (\$100 000 000), is only a moderate portion of the taxes which the Federal Government at present imposes on motorcar owners and the motor industry. In view of the \$15 000 000 000 quoted by Colonel Crosby[29] as having been spent on the existing highway system in the United States during the past 36 yr, \$3 000 000 000 does not appear out of proportion.

In connection with the more technical features of the paper, the writer wishes to express his appreciation to those who brought the latest

[60] *Civil Engineering,* March, 1937, p. 178.

[29] *Fortune Magazine,* August, 1936.

results of research and thinking before the members of the Society. At the time of the preparation of the paper (spring of 1935), the results of certain very illuminating and clarifying tests,[61] by R. A. Moyer, Assoc. M. Am. Soc. C. E., which have since been widely disseminated, had not come to the writer's attention, and he is grateful indeed for the addition of the material.

To simplify matters, and for convenience, it may prove desirable in covering the discussion, to follow the same order as used in the paper.

Dual Highway.—There is further opportunity for service in establishing design standards of the dual highway, because development has reached the interesting stage in which several variations of type are being constructed and subjected to the actual test of traffic. Further analysis of the various classes of accidents occurring on each type, and a thoroughgoing scrutiny of the comparative accident data, may clarify the situation and indicate the proper design to be used for certain particular conditions. Since the discussion did not record current design variations, or any analysis of the philosophy behind the use of particular designs, the writer takes the liberty of attempting to analyze the situation in order to define the issue more clearly in the hope that further thinking will be stimulated. The subject is so new, and there is so little supporting data, that the remarks should be considered only as a personal expression of opinion, and, therefore, fallible.

It is granted that a space separating opposing traffic is essential for trunk-line highways carrying any appreciable volume of traffic; but the width and treatment of this space is a very unsettled matter. There are cases when it is physically and financially limited to a width of a few feet, and other cases where it may easily be 150 ft or more. It is obvious that as the width increases, the chances of opposing accidents diminish. For the narrower widths it is clearly necessary to use some kind of curb to prevent impatient drivers from using the space for passing in case of traffic congestion. The treatment in use varies from straight-sided curbs, 6 to 11 in. high, to numerous types of sloping curbs.

When the width is only 3 or 4 ft, the vertical (batter of 6 on 1) curb may be the lesser evil, but it must be recognized that certain "elbow room" has been forfeited. The operator no longer has the chance to dodge sidewise the few feet necessary to avoid a side-swipe accident when he has been crowded or "pinched" by other vehicles. For widths of from 7 to 20 ft, some kind of sloping curb is indicated, the design to be such that it can be mounted at high speed without danger of loss of control. Considerable experimentation is being practiced in New Jersey in an effort to find a solution for this problem, and it is hoped that tests will be made, using different makes of cars operated at various speeds, in order to establish a design which will permit mounting without throwing the car out of control. A curb of improper design may cause a more serious type of accident than one produced by a side-swipe.

[61] *Bulletin 120*, Iowa Eng. Experiment Station, Iowa State Coll., Ames, Iowa, August, 1934.

Particular attention must be paid to the material between curbs. In wet weather, and during the spring break-up, unstabilized grass plots are very dangerous, the ground being so soft that vehicles are thrown out of control, if by any chance they are forced into the space while traveling at even moderate speeds.

For widths from 20 to 30 ft, a narrow mall or island may be placed in the middle, using smooth stabilized shoulders adjacent to and level with the pavement; and for widths greater than 30 ft, level shoulders with grass and landscaping between is indicated.

It is axiomatic that no form of fixed obstruction, such as poles, light standards, trees, etc., be placed in the dividing safety space. Headlight glare can be eliminated by planting a type of evergreen, the trunk of which will not exceed 2 or 3 in., and such a screen will also tend to act as a cushion for cars out of control.

As a motorist, the writer cannot but be tremendously impressed with the general design developed in Delaware; the operator has a feeling of tranquillity and utter safety not experienced in any of the narrow center-width and curbed designs. Passing is no problem because the motorist keeps to the right without supervision, and the wide, smooth shoulders provide a factor of safety as well as a space for parked vehicles.

It is the writer's belief that the width separating opposing traffic is a function of safe allowable speed. As the width decreases, the speed should decrease. Where it is necessary to reduce the width for financial reasons, the additional cost to the State of policing to insure reduced speed, as well as an appraisal of time losses, should be added to the financial analysis. (It should be remembered that the predominating reason for the safety record in the Holland Tunnel is the presence of police officers stationed at frequent intervals to enforce the regulations and prevent motorists from crossing from one lane to the other.) Where restricted widths are required, it may be necessary for the designing engineer to arrange with the State Police to provide sufficient officers to enforce compliance, rigidly, at all hours, with the safe speed established by the engineer. The public will not reduce speed unless an adequate number of officers are present to compel obedience.

Thus far there are no available data to support the idea that safe speed is a function of the safety island width or from which to deduce recommendation of the safe speed for particular widths and it is hoped that research will be instituted in order to establish basic principles.

Mr. Carpenter states that adoption of the dual type will not prevent overtaking accidents. That may be true, but it will greatly reduce them. On the ordinary two-way highway the presence of an oncoming car causes many such accidents, because the passing car is often forced to "crowd" the car being passed in an effort to avoid a head-on collision. This condition is entirely absent on a properly designed dual highway, and thus passing accidents should be reduced to a very small percentage.

Mr. Lewis is to be congratulated on his able presentation of the subject as related to urban regions. The discussion of the freeway is most

opportune, and highway engineers should read and digest all he has written with great care. The mention of the throttling effect of commercial development on the transportation efficiency of U. S. Route 1 in New Jersey is pertinent, and particular attention is directed to his statement respecting the reasonableness of barring access from property not directly assessed for the cost of highway improvements. The restriction of access should be no more serious than conditions imposed by the private railway lines traversing the United States.

In connection with Fig. 8, the writer wishes to inject a word of caution respecting the planting of trees adjacent to motor ways carrying high-speed traffic. He feels that no trees, other than small (2 in. to 3 in. in diameter) ornamental trees, should be planted within a line 50 ft from the shoulder of the highway. The car is not forcibly confined to a track, and often gets out of control. Many lives have been sacrificed on trees, and, therefore, these hazards should not be planted within range of the destructive effects of a motor car.

Colonel Crosby raises the interesting question as to the effect of the "blind spot" in rear vision on safely entering the main highway from the acceleration lane. There is no reason why manufacturers cannot solve the "blind spot" problem by the proper use of rear vision mirrors or periscopes, but since they have not, design practice must admit this flaw. The matter at once resolves itself into a choice between two evils.

The right-angle intersection is dangerous because vehicles wishing to turn usually approach at a higher rate of speed than practicable for the turn, thus causing the vehicle to "swing wide," usually across the first lane into the second. The entering car usually appears without warning, swings into the main road, effectively blocking both lanes, and, not having any great velocity in the direction of the main-road traffic, speed differentials are great. The only recourse of the motorist on the main road is to swing out on to the shoulder, but often the condition occurs so quickly that he has crashed into the entering car before he realizes the situation.

In contrast, the acceleration lane places the entering car in full view of the motorist on the main road for a period of several seconds, thus giving him sufficient time to move over into the left-hand lane. There is no temptation for the entering car to crowd over into the left-hand lane, and the speed differential of the two cars will not be great. The angle of approach is shown so small in Fig. 2 that cars on the main road will begin to appear in present-design rear-view mirrors before the entering car actually begins to encroach in the right-hand lane. Access lanes coming in at more of an angle than shown, will exclude rear-view mirror vision and, at the same time, any angle short of 90° will not provide full visibility for the motorist looking back through the side window.

The access road condition shown in Fig. 2 is probably no more hazardous than an ordinary turnout for passing; certainly it is less hazardous than weaving and passing to right and left as practiced on present multiple-

lane highways. This short statement is not intended to refute Colonel Crosby's well-warranted criticism, but only to bring out the comparative hazards of the situation.

Mr. Barnett suggests that the truck lanes be placed on the outside, with the high-speed lanes on the inside, in order to eliminate the expense of "fly-under" crossings. The condition is illustrated in Fig. 5 as contrasted with the original suggestion shown in Fig. 2.

The fundamental philosophy of the paper is that the highway should be designed for basic operating safety. The design shown in Fig. 5 requires the passenger vehicle to merge with the right-hand lane of truck traffic, pass across to the second lane, leave the second truck lane, and lastly merge with the passenger vehicles. This doubles the exposure to accident, particularly in passing across the truck lanes. The public is quite aware of the hazards entailed in maneuvering around trucks because of their speed, length, and size.

Access to inside truck lanes can be provided at an average expense of $100 000 even in flat country. Because the existing system of highways will carry all short-haul traffic and act as feeders to the express routes, truck access need not be spaced at intervals closer than 20 miles. Thus, such "fly-under" (under or over) connections will only add approximately $5 000 per mile to the cost, which does not appear out of proportion when the entire cost per mile of express highways is considered.

Superelevation and Curves.—The friction factor, f, has been added by several commentators in order to complete Equation (1). There is a difference of opinion among the commentators, however, as to the proper value of f to use, the discussion centering on the effect of centrifugal force acting on the driver and passengers. The problem in reality is divided into two parts by climatic conditions. In regions subjected to ice and snow, the low coefficient of tires on ice will govern; in warm climates, the effect of centrifugal force on the driver will govern. Because of the seriousness of side-skid on a curve at high speed, the writer feels that design should be conservative and that some factor of safety be provided in addition to that subsequently treated under the heading, "Factors of Safety."

Tests[61] indicate that skidding on ice occurs at friction factors ranging from 0.05 to 0.13. The writer has side-skidded on a curve at a friction factor of 0.04. It is recommended, therefore, that a friction factor of $f = 0.03$ be adopted for icy conditions up to the limiting speed that a car can operate on ice on a tangent. Superelevating a curve for icy conditions, for a speed greater than that at which a vehicle can operate on ice on a tangent, is unnecessary. Where ice is not a problem, centrifugal force on the driver will govern. Several recommendations have been made by the discussers, but the writer's tests confirm the recommendations of Mr. Wiley, namely, $f = 0.10$. The writer's experience coincides with that of Professor Moyer and Mr. Wiley, that a certain amount of skill not possessed by all drivers, is required to drive around a curve at a speed which develops a friction factor as great as 0.15.

In utilizing the foregoing recommended values, the radii of curves become so large that discomfort and possibly a hazard is experienced driving around them at the lower-speed ranges if the superelevation is a great as 1.25 in. per ft. It is recommended, therefore, that the transverse bank or slope be limited to 5%, provided, of course, the radii are expanded the proper amount based on the foregoing assumption. To illustrate, Fig. 10(*a*)

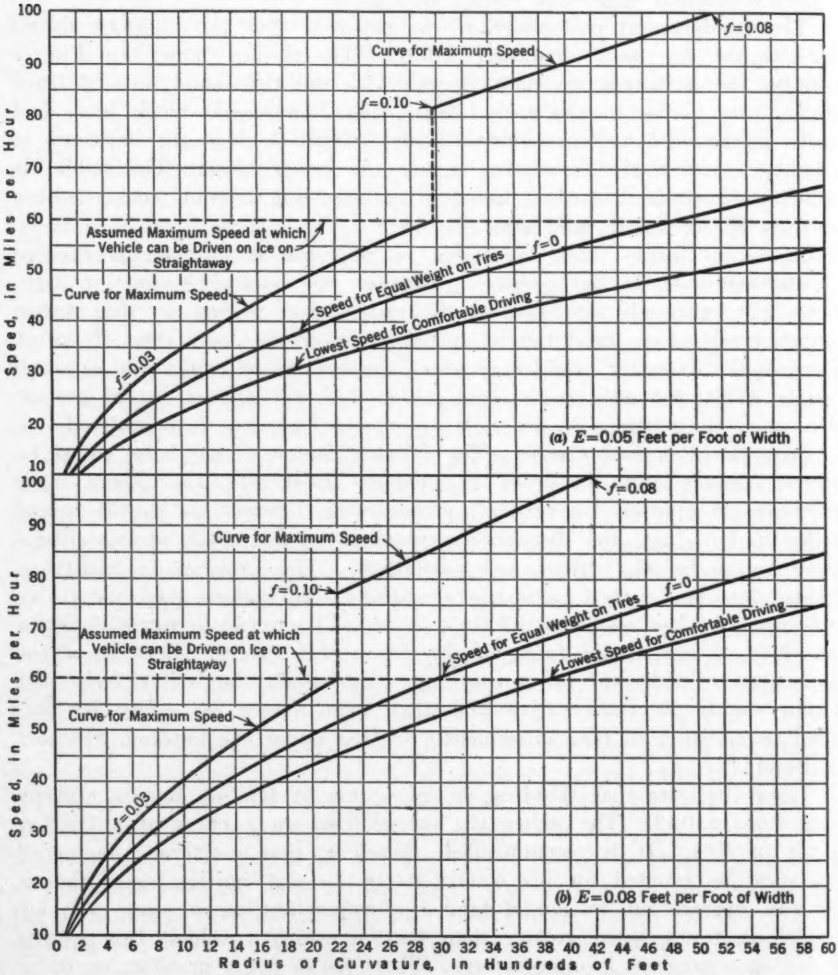

FIG. 10.—RELATION BETWEEN SPEED AND RADIUS OF CURVATURE IN REGIONS SUBJECT TO ICE AND SNOW

shows the speed-radius relation for one cross-slope, 5%, and the design curve is based on icy conditions governing only up to a speed of 60 miles per hr., on the theory that operation at greater speeds on ice, on a tangent, is impracticable. For speeds greater than 60 miles per hr, it

is based on friction factors varying (approximately) uniformly from a value of 0.10 to 0.08. The intermediate curve shows the speed-radius relation when the centrifugal force is exactly balanced by the superelevation. The lower curve, showing minimum speeds at which it is comfortable to operate, is based on the observation that it is fairly comfortable to operate for a limited period on a pavement on a tangent which is crowned 2 in. in 10 ft., and this amount of "unbalance" toward the inside of the curve is assumed as permissible. Fig. 10(*b*) is similar except that it is based on a superelevation of 8%, which is approximately 1 in. per ft. A study of these diagrams is instructive, especially as related to speeds below the "balanced" curve.

Before leaving this phase of curves, it will be illuminating to quote from a paper[52] by Professor Moyer, as follows:

"A significant feature of the slippage tests is the magnitude of the slippage as the speed of the car is increased. The trend indicates that the slippage at speeds of 80 miles an hour or more is so large on curves sharper than 3 deg [$r = 1\,910$ ft] that the most skillful driver will have difficulty in steering the car and holding it within a 10-ft traffic lane."

Transition Spirals.—The writer wishes at once to state that the necessity for transition spirals where high speed is concerned, has been conclusively demonstrated by the discussers. There appears to be no agreement, however, as to the proper length to use. In actual practice the rate of change of centrifugal acceleration ceases to be a criterion because superelevation cancels centrifugal force for a given superelevation-speed-radius combination. The factors affecting length, then, reduce to the time interval required for the operator to turn the wheel and the permissible rate of rotation. No one has commented on the time interval and it is hoped that engineers will continue to study this problem.

The profession is indebted to Mr. Haile for presenting his analysis of the principles of rotation as related to superelevation transitions. Such analyses are a step in the right direction, and it is suggested that investigation be continued to establish well substantiated values for the length, L_v, and the assumption that 0.27 represents a correct value for the expression, $\frac{2\,V^2_s\,(E_2 - E_1)}{L^2_s}$. The correctness of the result is dependent on the proper assumption of these values.

The writer is not so sure that railway-highway conditions are strictly analogous as related to superelevation transitions. The wheel-base of railway coaches is so great that a car will twist if the rate of change of superelevation is too rapid. The wheel-flange striking the outer rail is also a factor. Those conditions are not present in the highway problem. He also wishes to question the general assumption that very long spirals are desirable. It is possible that spirals longer than required to turn the steering wheel properly will require a nicety of "balance" and judgment in steering not possessed by all drivers. Thus, there may be

[52] *Civil Engineering*, February, 1937, p. 114.

a tendency for the car to "wobble" in the lane, due to the necessity of changing the steering wheel constantly, and developing different slippage angles. It is suggested, therefore, that additional data be accumulated for the purpose of establishing design practice.

Vertical Curves.—It is disappointing that no contribution was submitted on the problem of the influence of the head-light beam on the lengths of vertical curves. Mr. Haile states that the normal length of the head-light beam will not be affected if vertical curves provide a sight distance at summits equal to the distance required to stop the car. This becomes a reality if stopping distance is based on icy conditions. The general problem is divided into two parts, at sags and at summits.

At sags, the factors are, length of head-light beam, height of the lamp above the pavement, the vertical angle which the upper beam makes with the horizontal axis of the lamp, and the algebraic difference of the grades. With known factors, a graphical solution is simple and may be applied during the process of establishing profile grades. A mathematical solution is simple, but it requires solving simple equations for two cases in order to determine the limiting curve and is not as rapid, therefore, as the graphical method. The vertical curves required for this condition are not particularly long, but some designers may have to revise their ideas of vertical curve lengths somewhat. All motorists are aware of the experience at sags of having the head-light beam cut off by the ascending grade ahead, and seeing the light advancing up the pavement until the car reaches a point on the curve where the normal length of beam is restored.

At summits, the rounding curve intercepts the light rays and the down-curving pavement beyond this tangent point is in darkness, the head-lights piercing only space, giving the impression of a black wall ahead or that the car is about to plunge into an abyss. A mathematical analysis involving the differential calculus produces formulas (two cases), which yield startling figures. For instance, with an algebraic difference in the grades of 8% and a head-light beam of 400 ft, a vertical curve 2 100 ft long is indicated. A vertical curve of more than 3 000 ft is required, if a sight distance (during daylight), of 1 330 ft is desired, which illustrates the correctness of Mr. Haile's observation.

In view of the lack of corroborative evidence, it is hoped that tests and observations will be conducted for the purpose of establishing practical design procedure. Space does not permit, nor is it proper to present at this time, the development of the formulas mentioned. Mr. Haile states that passing would not be permitted at the summits of two-way highways. The writer knows of no positive method of preventing motorists from passing on summits, unless a police officer is stationed on every summit.

Grades.—Many commentators question the value of limiting maximum grades for the purpose of providing reasonable safety in descent on icy pavements. If traffic is so light that not more than two or three cars are present in any mile, safety in descent then affects the individual car and

that car only will be involved in a skidding accident, but where the traffic is heavy, with cars immediately adjacent to each other, a skid promptly involves other cars, with the result that from two to seven cars become snarled together, often with disasterous results. For instance, the Operation Department of the Port of New York Authority experiences difficulty as soon as ice forms on the short 4% grade of the New York approach to the George Washington Bridge, in spite of the fact that cinders are applied almost immediately. It has also been found necessary to close the Pulaski Skyway on Route 25, New Jersey, at certain times when ice has formed. The maximum grades on this structure are 3.5 per cent. Ice can form in a few minutes, and a maintenance crew with many miles to cover will find it difficut to spread cinders promptly. Consideration should also be given to the limiting friction factor of ice, which varies from 0.05 to 0.13.

It is true that, if locating engineers are looking for an "easy way out," the adoption of 8% and 10% grades will lessen their difficulties and shorten the time required for the location. This attitude is somewhat reminiscent of the early days of railway location. However, after the railways had passed from the pioneering period and became engaged in the reconstruction of their lines, the attitude disappeared. There are cases on record of the location work taking a considerably longer period of time to complete than the actual construction. Indeed, studies for some railway lines have extended over a period of ten years. Do highway engineers consider railway location more important than highway location, when the direct relation of alignment and grades upon vehicle safety is considered?

There should be a realization that the location of modern trunk highway routes is a very important matter, and all possibilities should be investigated in order that the best obtainable route and individual location be secured. If the physical conditions of a trunk route are of such a nature as to require 8% grades, that should be sufficient cause for its rejection, if it is in a climate subject to ice and snow.

Sight Distance.—Messrs. Barnett and Haile are quite correct in stating that minimum sight distance should not be computed based on 17.4 ft per sec per sec, because of the high friction developed between tires and road surface in order to decelerate at that rate This point should be particularly emphasized. Both suggest a friction value of 0.40, which is equal to a deceleration of 12.88 ft per sec per sec. This is greatly superior to the value given in the paper, and its use is recommended for climates not subject to ice and snow; consequently, Fig. 4 should be disregarded.

In regions subject to ice formation, the low friction coefficient of tires, on ice, should govern up to the maximum speed possible on such a surface. Because of the comparatively long distances resulting, and because the seriousness of an accident from this cause (within the lengths prescribed), is less than from insufficient superelevation, a friction value of 0.10 has been assumed. A new diagram, Fig. 11, showing stopping distances, has been prepared to be used instead of Fig. 4. The writer realizes that the

diagram may be criticized on the basis of showing distances that are too short to care properly for icy conditions in view of tests[51] which show coefficients on ice as low as 0.05.

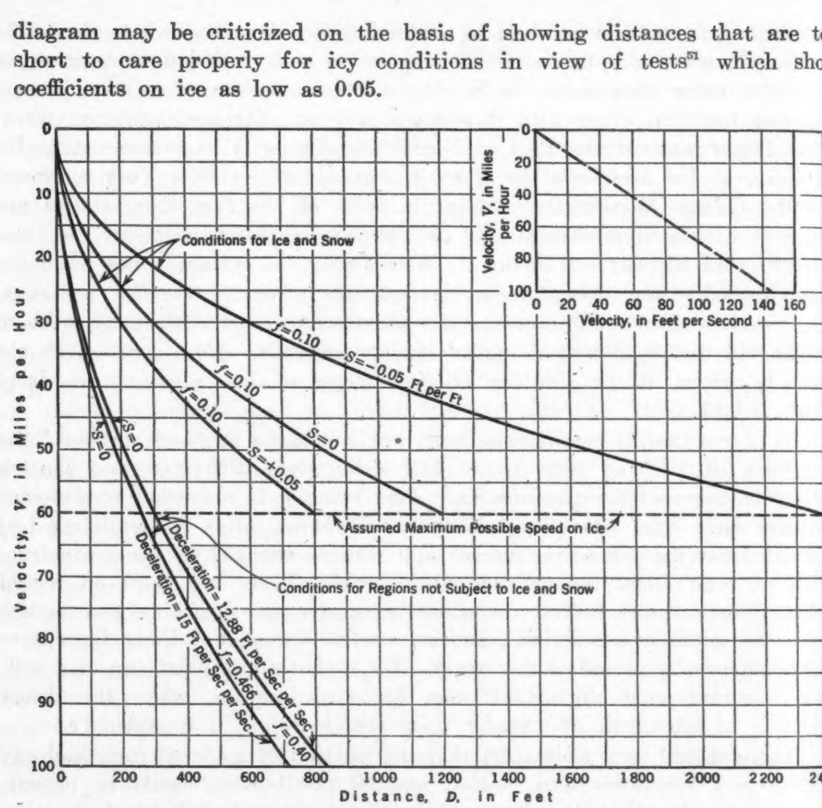

FIG. 11.—DISTANCE REQUIRED TO BRING A MOTOR CAR EQUIPPED WITH FOUR-WHEEL BRAKES TO A STOP AFTER BRAKES ARE APPLIED

The question may also be asked whether there is any real justification for designing on the basis of icy conditions in localities where ice may not occur more than a few days during each year. The answer to this question may be found in the well-stablished practice of other branches of engineering. A building, for example, may not have to withstand the wind loads for which it is designed more than once in 10 yr, or flood-control dams, levees, and spillways may not be called upon to withstand full flood height for which these works are designed, more than once in 20 yr; yet engineers feel it is sound policy to design for the maximum condition. In this connection it may be stated that highway engineers apparently are more flood conscious than accident conscious, as it is quite usual to construct highways above the highest recorded flood level.

It is to be noted from Fig. 11 that a speed of 60 miles per hr under icy conditions, requires a greater stopping distance than a speed of 100 miles per hr, with dry and wet surfaces. For a speed of 60 miles per hr on ice, a sight distance of 1 332 ft (1 200 ft + 132 ft) is required on level grade for a one-way (dual) highway. In order to obtain this

sight distance on a curve (with a central angle greater than 14°), and assuming that all obstructions are cleared for a distance of 40 ft from the edge of the pavement, a curve with a radius greater than 5 300 ft, will have to be adopted. In other words, if all trees, cut banks, or other obstructions are kept uniformly 40 ft from the edge of the pavement, it will be necessary to adopt a curve of not less than 5 300-ft radius in order to obtain a sight distance of 1 332 ft.

Thus, in general, it is evident that if the design is based on icy conditions, sight-distance requirements will compel the use of curves of sufficient radii to care for speeds of 100 miles per hr.

Safety on Tangents.—There has been an implied assumption by some discussers that a tangent should be considered as a criterion of safety, merely because it is straight. Straightness alone does not assure safety. The mere fact that there are more miles of tangent than curves is not the only reason that more accidents occur on tangents than on curves. There are sound reasons for these accidents. Often a fairly long tangent is encountered after a series of sharp curves. This is an invitation to the motorist to speed, and frequently other operating conditions not connected with straightness are such that there is no justification for an increase in speed.

A tangent with no separation between opposing traffic, narrow soft shoulders, deep ditches, heavy crown, steep grade, and with poles, trees, head walls, bridge walls, and guard rails near the pavement, is surely more dangerous than a properly designed curve. Such a tangent, however, is in direct contradiction to the spirit of the paper. An analysis of accidents which can occur on a tangent of this nature may be enumerated as follows: Head-on collision; rear-end collision, with car standing on the pavement; side-swipe due to passing; forced off road by passing car and overturning in ditch; car sufficiently out of control due to soft shoulder to collide with tree, utility pole, guard fence, or other obstruction; car skidding with the aforementioned results; and a blow-out or other mechanical failure, resulting in all the foregoing types of accidents.

With separated roadways, wide smooth stabilized shoulders free of all obstructions, adequate sight distance, smooth profiles, and reasonable grades, some types of these accidents would be eliminated entirely, and the seriousness of a large number of accidents would be minimized. Sufficient time, space, and "elbow room" would permit a vehicle to recover from a minor mishap in time to prevent disaster.

It is not contended that a properly designed curve of sufficient radius, adequate superelevation, with well designed spirals, is more dangerous than any other part of the road structure. The philosophy of the paper is that hazards should be removed from the highway whether they occur on a tangent, on a curve, in a tunnel, on a bridge, in urban regions, or in the open countryside.

Statistics.—Several discussers presented detailed accident statistics and have deduced certain conclusions therefrom. Statistics may prove misleading unless thoroughly analyzed by a competent statistician. With no

attempt to treat a subject with which the writer is unfamiliar, it may be pertinent, however, to point out certain of the more obvious pitfalls connected with traffic-accident statistics. In analyzing such records, the relative values that must be assigned to certain figures should be borne clearly in mind. For instance, unless it is realized that less vehicle miles are operated at night than during the day, the erroneous conclusion might be reached that it is less hazardous to motor at night than during the day, when it is noted in Table 5 (*f*) that 58% of all accidents occur during daylight, whereas only 42% occur during dusk and darkness. To particularize further, it must be realized that there are less vehicle miles operated during rain, fog, ice, and snow, that certain regions in the United States have little or no rain, certain sections have no ice, and, furthermore, that there are fewer hours of rain than dry weather, less hours of snow and ice on the pavement, and still less hours of actual snowfall and fog. As previously noted, there are more miles of tangent than curves.

When the statistics are properly adjusted to represent the true relation on a basis of comparison of hours-mileage-volume, a clearer picture of the actual exposure to accident for given conditions will result. There is little doubt that it can be shown that the exposure to accident is increased by rain, fog, snow, and ice, and that these are definite factors which should enter into the formula for safe highway design.

Capacity.—Mr. Diehl states correctly that the safe spacing of cars should be the distance required to bring the vehicle to a stop (braking distance + lag distance). He further states that because of this, the capacity of a highway is unduly reduced at the higher speeds. This is true, but not to the extent which some investigators have assumed, since their assumption does not coincide with the driving habits of most motorists. Without condoning or in any way recommending this dangerous practice, it must be realized that operators usually keep a distance back from the car ahead only sufficient to care for the space covered during the lag period. The motorist relies on being able to decelerate at the same rate as the car in front. If a lag period of 1.5 sec is assumed, this distance approaches the braking distance at the lower speeds, but is only a fraction of the distance at the higher speeds. This becomes very striking if the total stopping distance is computed for icy conditions.

Factors of Safety.—A careful study of Figs. 10 and 11, shows that the factor of safety for sight distances and curves is either zero or very low if the design for the actual expected speed is based on these diagrams. As Professor Hawthorn states, "the driver's control over his vehicle varies, other conditions being equal, with the square of the speed." In a rough sense, this relationishp can be used as a measure of factors of safety. In other words, if Figs. 10 and 11 are used in the sense that values derived therefrom represent the ultimate strength of a material, then established engineering practice requires that some factor of safety be applied. If the "ultimate strength" is assumed as 100 miles per hr, then a factor of safety of 1.5 is provided at a speed of 80 miles per hr, 2 at 70 miles per hr, and somewhat less than 3 at 60 miles per hr. It should be remembered

that a factor of safety (against ultimate strength) of 3 is in established use for such a uniform and consistent material as structural steel. Fully realizing that such a method is not strictly scientific as applied to highways, and yet it does form some measurement of safety.

If, then, it is desired really to design a highway for safe operation for a speed of 60 miles per hr, curves, spirals, superelevation, and sight distance (except where icy conditions require greater values), should be designed on the basis of a speed of 100 miles per hr. This will give a measure of safety in a longitudinal direction.

Safety in a lateral direction is less easily measured. It is disappointing that the discussion did not contain any data and suggestions from which a start could be made to appraise factors of safety in a lateral direction. For instance, if the pavement was flanked by smooth, stabilized shoulders of infinite width, with all obstructions removed, complete lateral safety would be provided for many types of "out-of-control" skidding and "driver-asleep" accidents. Conversely, if the shoulder were only 1 ft wide, the accident hazard would be greatly increased. Manifestly, it is utterly impossible to provide shoulders of infinite width, but there must be some measure of the factor of safety for any given speed-shoulder-width combination. This problem is not simple, and, therefore, presents a challenge.

In this connection, certain engineers have assumed that once a vehicle is out of control, the responsibility of the highway designer ceases. If this assumption were logical, then from force of logic the conclusion would be reached that the operator should always drive perfectly, in which case there would be no highway problem. Since it is physically impossible for the motorist to drive perfectly, and since the results of an accident are the same from whatever cause, the designer does have a responsibility and, therefore, should make a reasonable allowance for the case in which the car is out of control.

For example, as Mr. Lavis suggests, somnolence is a factor which should be considered. Any one may be guilty of it, and a certain amount of protection should be provided. One-way roadways, with sufficient width between, furnish a certain degree of protection to the left of the motorist; the shoulder furnishes protection to the right. The slight additional roughness of the shoulder will awaken most drowsy drivers, and if given space and time, the car can regain the pavement without mishap. The same conditions apply to inattentive operators.

The statements of Messrs. Carpenter and Wiley should be emphasized, that the minimum standard often becomes "the" standard, and is used when better standards could be applied with little, if any, addition in cost. Many times the designer is so engrossed in solving structural or mathematical problems that he becomes "blind" to the essential issue of designing for basic operating safety, and, consequently, unwittingly inserts unnecessary hazards to operation. Often it is no more expensive to design safely, but there are many times when safe design requires courage and conviction on the part of the designer. Professor Alvord has performed

a service in emphasizing that under-design is unprofessional. It should be repeated that consistency in design is the very soul of safe design.

The highway should be considered as a unit, and uniformity and consistency should be a feature of the results, whether the route be in the city, in a tunnel, on a bridge, or in the open countryside, to the end that it efficiently and safely serves its function as an element of transportation.

Application to Broad Field.—Mr. Carpenter is acute and is quite correct in his assumption that the writer's interest in safety also extends beyond the limits of the express highway. It covers the entire highway field, including farm roads and city streets, not only as related to new construction, but also to the removal of hazards from the existing system.

Design in this broad field must be approached with new vision, and enlightened standards must be established, based on a definite design speed, to the end that operating safety is the basic philosophy. As stated in the paper, the selection of the design speed for any particular class of road is one of the most important decisions connected with the design. The future possibilities of use and traffic volume must be determined accurately in order that adequate right of way and proper alignment be obtained. With such provisions, the initial design speed for remote roads may be set less than the ultimate as far as profile, graded width, type of pavement, and details are concerned, if economy is paramount. With such a procedure, speed standards may be improved from time to time without appreciable loss of investment. If the speed standard is less than the legal limit, the permissible rate of speed should be posted at frequent intervals.

There is a tremendous opportunity to perform a humanitarian service in the removal of hazards from the existing system of highways. The magnitude of the task is apparent when it is remembered that there are more than 3 000 000 miles of rural roads in the United States. There is crying need for a hazard survey to locate and classify hazards according to some rating; and a program with appropriations in each State and each political sub-division of each State for the continued and consistent removal of hazards. The removal of such death-traps is quite certain to yield greater dividends in lives saved than the complete removal of all railway grade crossings.

In order to provide a criterion of operating safety, it is suggested that the entire existing system be classified on the basis of safe allowable speeds. Thus, for example, it may be established that a given section between towns, or other natural division points, may permit vehicles to operate safely at a speed of, say, 35 miles per hr, except for a few particular locations where the safe speed may drop to a lower figure, say, for illustration, 20 miles per hr. These places, then, are of particular hazard, and every effort should be made to reconstruct them to the 35-mile standard at the earliest possible date. Except as related to the elapsed time of travel, it is inconsequential what the safe speed may be—10 miles per hr, or 60 miles per hr—as long as consistency is maintained. It is the unexpected hazard that will continue to take the lives of many conserva-

tive motorists on the existing system. The safe speed should be posted as a speed limit on all roads which are of a lower standard than the legal limit, and a gradual change in operating conditions should be provided when passing from one speed classification to another.

The writer realizes that the foregoing attitude is, in part, somewhat contradictory to the spirit expressed in the paper, but the paper applied to new construction, particularly as related to the trunk system. It is manifestly impossible to reconstruct all roads to high-speed standards. A program such as that outlined herein will do much to relieve the accident situation within a relatively short period, certainly before any large volume of new construction can be completed. These roads will always be feeders to the trunk lines.

Expensive Highways not Necessarily Safe.—Certain engineers appear rather complacent with respect to present and past highway-design thinking, and seem to feel that further mental and physical effort is not only unnecessary but somewhat puerile. In the interest of safety, and for that purpose only, it may prove educational if a few instances of expensive projects, which fall short of the philosophy of the paper, are given. Their identity is unimportant since no reflection on the designers is due or intended, and the illustrations are given solely to show that the engineer has not yet brought his thinking up to date as related to safety—he is not yet thoroughly safety conscious.

An express highway, several miles long, was constructed, which cost nearly \$5 000 000 per mile, without right-of-way cost. It carries a daily traffic of approximately 45 000 passenger automobiles, trucks not being permitted; yet it has center ramps, the light standards are placed in the center safety island, and although the general posted speed is 35 miles per hr, in several places the posted speed is 20 miles per hr, because of sudden sharp curves and center ramps.

An elevated structure several miles long was constructed at a cost of more than \$5 500 000 per mile. There is no physical separation of opposing traffic; it has center ramps, and the roadway has an odd lane. The economic justification for this structure, to a great extent, was based on its carrying a large volume of trucks. Present traffic is approximately 35 000 vehicles daily. During the first six months of operation the accident situation became so serious that trucks were excluded from the structure in an effort to reduce the accident toll.

This structure is a small part of a much larger project which cost an average of more than \$1 000 000 per mile. The greater part of the route is of multiple-lane type, four to six lanes wide, constructed at grade in the usual manner, except that all railway and important highway crossings are eliminated by grade separation. When completed, it was considered an outstanding example of modern highway design. Within a year after being opened to traffic, the newspapers and public christened it "Death Highway." Three-car and four-car accidents were common, and six-car and seven-car accidents not uncommon. In the last two or three years, a large mileage has been reconstructed and made over into a divided way, with an island sepa-

rating opposite direction traffic, at an additional cost of approximately $65 000 per mile. It is reported that accidents have decreased where the roadways have been separated.

These projects were designed by some of the most competent and alert highway engineers in the country, who were using the most modern and up-to-date design methods in vogue at the time. When it is considered that the work was performed within the past decade, the swiftness with which highway design thought is developing becomes apparent. It is also apparent from the unfortunate experience noted that the public is awakening to the situation, and unless the engineer becomes alive to the problem, he is quite likely to find himself in an uncomfortable position from which he may not be able to extricate himself without loss of prestige.

Conclusion.—There has been an assumption on the part of some commentators that the highway engineer is unduly restricted by laymen in the form of commissioners and politicians, and, therefore, of necessity must continue to design "within the money" in accordance with the ideas of other than technical men. This is true to a certain extent, even at the present time, and it indicates that engineers should rise to the occasion and make an intelligent effort to bring the true facts before governing bodies. It is also true that the layman will only value the engineer and his judgment as highly as the engineer values and has confidence in his own competence and judgment. There was a time when engineers designed bridges "within the money" because they were brow-beaten into it against their better judgment. A series of failures convinced the governing bodies that they did not know as much about the forces of Nature as engineers. As a consequence, it is very rare for a bridge to be badly under-designed in this day. The highway is failing to-day, but its failure is not as spectacular and obvious as a bridge failure.

It has been the writer's observation that governing authorities are willing to listen to, and act on, concerted engineering opinion when they are convinced of the hazards involved in disregarding it; but when the engineers are uncertain and not in agreement among themselves as to the proper method of solving a given problem, the governing bodies must not be blamed too much if they take a financially conservative, as well as a politically expedient, attitude.

Consequently, it is of the utmost importance for engineers to bring their ideas more in harmony with each other, and having achieved solidarity, to go forth and spread the gospel of highway safety in relation to design. In the last analysis the engineer will be held responsible by the public for safe highway design. The writer reiterates his conviction that the engineer must have vision and courage.

The paper has been broadened and enriched by the discussion, and, in closing, it is desired to express again appreciation to all the discussers who so generously gave their time and effort to the subject.

In addition to those noted previously, acknowledgment is freely given to the Automobile Manufacturers' Association, and Mr. C. George Krueger, for assistance and courtesy in supplying data and illustrations.

AMERICAN SOCIETY OF CIVIL ENGINEERS

Founded November 5, 1852

DISCUSSIONS

SELECTION OF MATERIALS FOR ROLLED-FILL EARTH DAMS

Discussion

By L. F. Harza, M. Am. Soc. C. E.

L. F. Harza,[36] M. Am. Soc. C. E. (by letter).[36a]—Much credit is due the author for crystallizing the technique of selecting ideal material for the impervious part of an earth dam in so far as this can be done by mechanical analysis. The paper is excellent with reference to the one problem of ideal grading of the impervious region, without relation to the remainder of the dam nor to its foundation nor abutments, upon the assumption that such ideal grading is consistent with other attendant facts and conditions.

In a practical application, the ideal material is seldom available in the immediate vicinity. Usually, a large part, if not all, of the dam must be built of less ideal materials in order that, for reasons of economy, it may be obtained from necessary excavations or near-by sources. One would rarely find a foundation upon which to build that would conform to the ideal grading and degree of compaction called for by the author. Furthermore, it may be contended that the word, "stability," in the sense in which it is used when applied to a given material, is an entirely different word from "stability" when applied to the dam as a whole, which is almost synonomous with "safety"; and the two uses of the word may have little in common. The unstable core of a hydraulic fill is made stable with relation to the dam as a whole by sufficiently heavy and free draining slopes. The same principle can be applied to a rolled fill to protect the up-stream slope from damage by rapid lowering of the reservoir level. Whether or not the ideal grading should be struggled for with much consequent increase in cost depends upon the purpose of the

Note.—The paper by Charles H. Lee, M. Am. Soc. C. E., was published in September, 1936, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: December, 1936, by Messrs. T. T. Knappen, and Paul Bauman; January, 1937, by Messrs. William C. Hill, A. Floris, and Fred D. Pyle; February, 1937, by Messrs. Joel B. Cox, Stanley M. Dore, John E. Field, William P. Creager, and Joseph Jacobs; March, 1937, by Messrs. C. H. Kadie, Jr., and Ralph Bennett; and April, 1937, by Ralph R. Proctor, M. Am. Soc. C. E.

[36] Cons. Engr. and Pres., (Harza Eng. Co.), Chicago, Ill.

[36a] Received by the Secretary March 25, 1937.

dam, the foundation and abutment conditions, and the value of water, drainage provisions, and other factors.

It is quite natural that the author's insistence upon tightness should be emphasized in California where water conservation is of such importance, and because soluble elements are common in soils of arid regions; but a vast number of flood-control dams are likely to be built in the East where tightness beyond the requirements of safety may have no importance whatever. Obviously, it would be unnecessary to struggle for high impermeability, for example, of mere retarding dams for flood-control purposes, such as those of the Miami Conservancy District. Dams of high permeability may be just as safe and stable as very tight dams: (*a*) If the materials of different character are disposed in proper relation so that they become progressively more permeable (and amply so) toward the toe; (*b*) if each material will not filter through the next material in succession; (*c*) if the materials are not soluble; and (*d*) if ample natural drainage is provided on the filter principle to insure the escape of leakage without the carrying out of material. Each material, of course, should be compacted as much as its character permits.

It would be superfluous to struggle for the ideal grading and tightness of a dam if the foundation itself were subject to leakage, dwarfing that of the dam. This is the usual case with reference to dams in the Southern Peninsula of Michigan, some of which rest on as much as 1 500 ft of porous sand and gravel, and depend for stability upon proper drainage. Seepage is dangerous only to the extent that it is not properly handled. The art or science of draining engineering structures, especially dams, is of such importance as to deserve recognition as almost a distinct field of engineering which has received altogether too little attention.

The logical design of a dam and the selection of materials, even for the impervious part, is inseparable from its foundation and abutment conditions, drainage possibilities (whether natural or artificial), importance of water conservation, and the purpose and location of the dam. It is well to lean toward the ideal as far as consistent with these conditions.

AMERICAN SOCIETY OF CIVIL ENGINEERS

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DISCUSSIONS

ECONOMIC DIAMETER OF STEEL PENSTOCKS

Discussion

BY MESSRS. JOSEPH D. LEWIN, F. KNAPP, AND RALPH W. POWELL

JOSEPH D. LEWIN,[25] JUN. AM. SOC. C. E. (by letter).[25a]—In that it permits taking into consideration such new factors as bend losses, entrance losses, etc., the development of penstock analysis in this paper is a very interesting one. In the form in which the authors have presented their analysis its application is limited to uniform (that is, horizontal) penstocks. A few changes in the interpretation of the present analysis are sufficient to extend its application to all horizontal and inclined penstocks.

Friction Loss and Annual Cost of Penstock.—In 1914, Professor A. Ludin published his formula[26] which, in the notation of the paper, may be written:

$$D = \frac{\sqrt[7]{101.3\, e_T\, e_G\, e_j\, s_z\, b_w\, T}\;\sqrt[7]{\dfrac{(Q')^3}{H}}}{(1+i)\,\gamma\, C^2_z\, a_s} \qquad (43)$$

in which e_T = the efficiency of the turbines, in decimals; e_G = the efficiency of the generators, in decimals; e_j = the efficiency of the joints, rivets, etc., in decimals; s_z = allowable stress, in kilograms per square centimeter; b_w = the value of the power at the generators, in marks per kilowatt-hour; T = hours of use; γ = the specific weight of steel (= 7.9); C_z = the friction coefficient in the Chezy formula; a_s = the annual cost of a ton of steel, in marks per ton; and Q' = equivalent discharge, in cubic meters per second. Converted into the units of this paper, Equation (43) becomes:

$$D = 0.56682\,\sqrt[7]{\frac{e\, b\, s_g\, e_j\, T}{a\, r\, C^2_z\,(1+i)}}\;\sqrt[7]{\frac{(Q')^3}{H}} \qquad (44)$$

NOTE.—The paper by the late Charles Voetsch, M. Am. Soc. C. E., and M. H. Fresen, Assoc. M. Am. Soc. C. E., was published in November, 1936, *Proceedings*. Discussion on the paper has appeared in *Proceedings*, as follows: March, 1937, by Messrs. R. A. Monroe, William E. Rudolph, and Peter Bier; and April, 1937, by Adolpho Santos, Jr., Assoc. M. Am. Soc. C. E.

[25] Hydr. Designer, Phœnix Eng. Corp., New York, N. Y.

[25a] Received by the Secretary March 11, 1937.

[26] "Die Wirtschaftliche Bemessung von Triebwasserleitungen" von A. Ludin, *Zeitscrift für das gesammte Turbinenwesen*, 1914, No. 13; also, "Wasserkraftanlagen", von A. Ludin, Berlin, 1934, pp. 194–197.

The differences in the root, in the constant, and in the friction coefficient are due to the use of different equations for determining the friction losses.

The authors used the Scobey formula, Equation (1), and Professor Ludin, the Chezy formula,

$$h_f = \frac{4\ V^2}{C^2_z\ D} \qquad (45)$$

For turbulent flow with rough walls, the exponent, 2, for the velocity seems preferable.

For the penstock under consideration the economic conditions of location, elevation, transportation, etc., determine the first radical in Equation (44) and the corresponding quantity in Equation (11), so that they become constant, which may be expressed, respectively, by the symbols, K'_r and K_r. The economic diameter can be expressed as:

$$D = K_r \sqrt[6.9]{\frac{Q^{2.9}}{H}} = K'_r \sqrt[7]{\frac{Q^3}{H}} \qquad (46)$$

For pressures greater than 300 ft, and for the economic conditions prevailing in Central Europe, Professor Ludin[26] developed the formula:

$$D_m = \sqrt[7]{\frac{5.2\ Q^3}{H}} = 1.26557 \sqrt[7]{\frac{Q^3}{H}} \qquad (47)$$

in which D_m = the economic inside diameter, in meters. The solution of Equation (47) is greatly facilitated by the preparation of a nomogram such as Fig. 2. Equation (11) may seem very complicated, and it will be quite difficult to obtain correct data for the value of the power and costs of the penstock. The value of K_r or K'_r, however, remains nearly unchanged. If the steel costs, a, are greater, the price of power will increase accordingly; or, if the load factor, F, is small (peak power plant), the price of the peak energy, b, will be high too.

This is the reason why Equation (47) is correct for most European penstocks. For all cases with pressures less than 300 ft (100 m) Dr. Bundschu[27] advises heads of 300 ft. Using the values given in the paper under the heading, "Use of Formulas," as prevailing in the United States, Equation (11) becomes:

$$D = 1.33822639 \sqrt[6.9]{\frac{f\ Q^{2.9}}{H}} \qquad (48)$$

The corresponding diameters can be obtained from Fig. 2 which must be changed accordingly. In Fig. 2, there are two scales for diameters, the left for the European conditions (Equation (47)), and the right for the American conditions (Equation (48)). In the computation the writer prefers to use "the equivalent discharge," Q'_e, (discussed subsequently),

[27] "Wirtschaflicher Entwurf von Turbinenrohrleitungen", von Bundschu, *Wasserkraft und Wasserwirtschaft,* 1931, pp. 56–58.

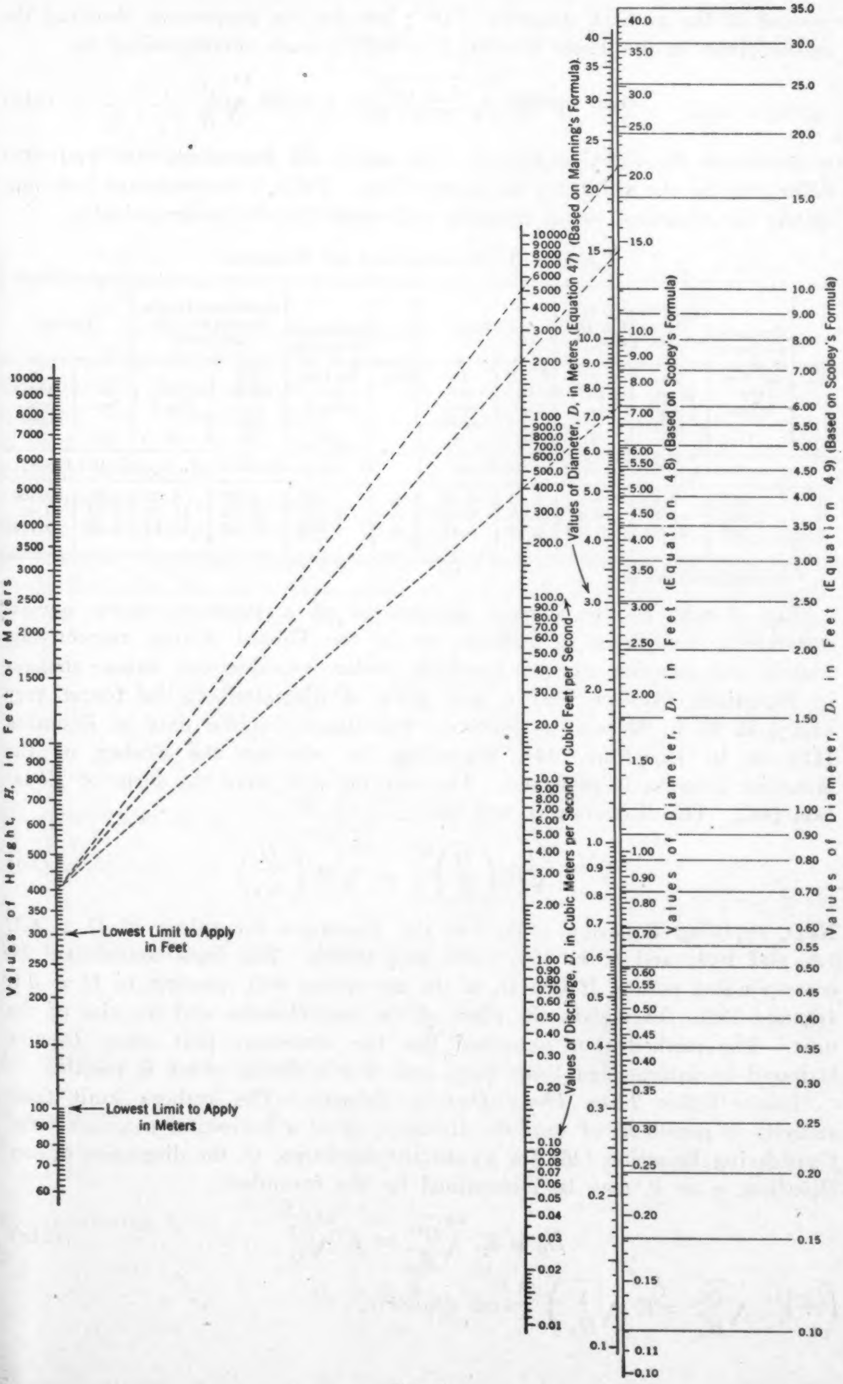
Values of Height, H, in Feet or Meters
Lowest Limit to Apply in Feet
Lowest Limit to Apply in Meters
Values of Discharge, D, in Cubic Meters per Second or Cubic Feet per Second
Values of Diameter, D, in Meters (Equation 47) (Based on Manning's Formula)
Values of Diameter, D, in Feet (Equation 48) (Based on Scobey's Formula)
Values of Diameter, D, in Feet (Equation 49) (Based on Scobey's Formula)

FIG. 2

instead of the authors' quantity, $f\,Q^{2.9}$; but for the purpose of checking the values given in the Table 3, with $f = 0.25$, a scale corresponding to,

$$D = 1.38823 \sqrt[6.9]{\frac{0.25\,Q^{2.9}}{H}} = 1.13555 \sqrt[6.9]{\frac{Q^{2.9}}{H}} \ldots\ldots\ldots\ldots (49)$$

is drawn on the right in Fig. 2. The scales for Equations (48) and (49) differ only by the difference in the constant. Table 5 demonstrates how very closely the computed values coincide with those found nomographically.

TABLE 5.—COMPARISON OF METHODS

Item No.	Maximum discharge, Q, in cubic feet per second (1)	Economic Inside Diameter, D, in Feet: By Equation (48) (2)	Economic Inside Diameter, D, in Feet: By Fig. 2 (3)	Error: In feet (4)	Error: In percentages* (5)	Item No.	Maximum discharge, Q, in cubic feet per second (1)	Economic Inside Diameter, D, in Feet: By Equation (48) (2)	Economic Inside Diameter, D, in Feet: By Fig. 2 (3)	Error: In feet (4)	Error: In percentages* (5)
(a) Average Head, H, Equals 100 Feet						(b) Average Head, H, Equals 400 Feet					
1	400	7.40	7.35	0.05	0.68	4	400	6.03	6.02	0.01	0.166
2	2 000	14.49	14.41	0.08	0.555	5	2 000	11.85	11.90	0.05	0.42
3	5 000	21.30	21.20	0.10	0.47	6	5 000	17.42	17.40	0.02	0.115

* All less than 1 per cent.

Fig. 2 will be the correct dimensions of a penstock under normal economical conditions in Europe, or in the United States, respectively. Should the location of the penstock under consideration cause changes in Equations (47) or (48) a new scale of diameter can be found very easily, in 25 to 30 min, as follows: Substitute the new data in Equation (11) or in Equation (44), depending on whether the Scobey or the Manning formula is preferred. The solution will have the form of Equation (46). The discharge, Q, will be:

$$Q = \sqrt[2.9]{H\left(\frac{D}{K_r}\right)^{6.9}} = \sqrt[3]{H\left(\frac{D}{K'_r}\right)^{7}} \ldots\ldots\ldots\ldots (50)$$

Next, applying Equation (50), find the discharges for values of $D = 0.1$, 1.0, and 10.0; and $H = 100$, 1 000, and 10 000. The lines connecting the corresponding points, H and Q, in the nomogram will intersect in $D = 0.1$, 1.0, and 10.0. This gives the place of the new D-scales and the size of the unit. The method thus described has the advantage that every D-point is found by intersecting three lines, and thus a double check is possible.

Losses Other Than Those Due to Friction.—The authors limit their analysis to penstocks of uniform diameter, or to a horizontal penstock only. Considering Equation (46) for a constant discharge, Q, the diameters at any elevation, u or v, may be determined by the formulas:

$$D_u = K_r \sqrt[6.9]{\frac{Q^{2.9}}{H_u}} = K \sqrt[6.9]{\frac{1}{H_u}} \ldots\ldots\ldots\ldots (51a)$$

$\left(\text{or } K'_r \sqrt[7]{\frac{Q^3}{H_u}} = K \sqrt{\frac{1}{H_u}}\right)$; and similarly,

$$D_v = K \sqrt[6.9]{\frac{1}{H_v}} \quad \text{.......................(51}b\text{)}$$

$\left(\text{or } K \sqrt[7]{\frac{1}{H_v}}\right)$ in which $K = K_r \sqrt[6.9]{Q^{2.9}}$ (or $K'_r \sqrt[7]{Q^3}$). The relation between two diameters at the different elevations may be expressed as:

$$D_v = D_u \sqrt[6.9]{\frac{H_u}{H_v}} \quad \text{.......................(52)}$$

$\left(\text{or } D_u \sqrt[7.0]{\frac{H_u}{H_v}}\right)$

In an inclined penstock, therefore, the different diameters, D_v, are determined by Equation (52) after one diameter, D_u, has been computed. Consequently, Equations (27) and (33) of the paper can be used without applying the limitations fixed by the authors. This general application of bend-loss formulas is the new idea introduced by the authors in the analysis of penstocks.

Example.—It is intended to design a penstock, a part of which is horizontal and a part inclined. The angles of bends, and their elevations, are known. Applying Equation (52), reduce the different diameters to a single diameter, D_u, at a basic plane of reference, Elevation u, the result is a series of formulas for annual friction loss, as follows (see Equation (19)):

$$E_{f1} = \frac{1.17631\ K_s Q^{2.9} e f b L_1}{\left(D_u \sqrt[6.9]{\frac{H_u}{H_1}}\right)^{4.9}} = \frac{1.17631\ K_s\ Q^{2.9}\ e f b}{\sqrt[6.9]{H^{4.9}_u}}\ \frac{L_1 \sqrt[6.9]{H^{4.9}_1}}{D^{4.9}_u} \quad \text{....(53)}$$

or, substituting $\kappa_f = \frac{1.17631\ K_s\ Q^{2.9}\ e f b}{\sqrt[1.41]{H_u}}$:

$$E_{f1} = \kappa_f\ L_1\ \frac{\sqrt[1.41]{H_1}}{D^{4.9}_u} \quad \text{.....................(54)}$$

For any inclined part of the penstock the annual cost of friction loss will be:

$$E_{fn} = \frac{\kappa_f\ L_n \sqrt[1.41]{H_n}}{D^{4.9}_u} \quad \text{.......................(55)}$$

for any bend loss, at any elevation:

$$E_{b1} = \frac{18.70719\ Q^3\ e f b}{\left(\sqrt[6.9]{\frac{H_u}{H_1}}\ D_u\right)^4}\ K_{b1}$$

or substituting $\kappa_b = \frac{18.70719\ Q^3\ e f b}{\sqrt[1.72]{H_u}}$:

$$E_{b1} = \frac{\kappa_b\ K_{b1} \sqrt[1.72]{H_1}}{D^4_u} \quad \text{.......................(56)}$$

In a penstock consisting of the three sections shown in Fig. 3, the total annual cost of the entire penstock will be the sum of a series of formulas similar to Equations (55) and (56), thus:

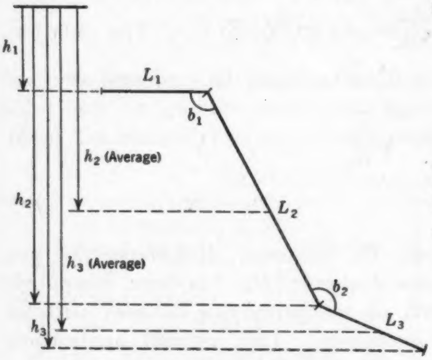

FIG. 3.—SCHEMATIC LAYOUT OF THE PENSTOCK

$$E_{\text{Total}} = E_{f1} + E_{f2} + E_{f3} + E_{b1} + E_{b2} + E_p + E_e \quad \ldots\ldots(57)$$

The economic diameter is determined by the first differentiation of Equation (57); thus:

$$\frac{dE_t}{dD_u} = -\frac{\kappa_f\, 4.9}{D^{5.9}{}_u}\left[\sqrt[1.41]{H_1}\,L_1 + \sqrt[1.41]{H_2}\,L_2 + \sqrt[1.41]{H_3}\,L_3\right]$$

$$-\frac{\kappa_b\, 4}{D^5{}_u}\left[\sqrt[1.72]{H_1}\,K_{b1} + \sqrt[1.72]{H_2}\,K_{b2}\right] + B = 0 \quad \ldots\ldots\ldots\ldots(58)$$

in which B is defined by Equation (23*d*). Substituting,

$$U = \frac{4.9\,\kappa_f}{D^{5.9}{}_u}\left[\sqrt[1.41]{H_1}\,L_1 + \sqrt[1.41]{H_2}\,L_2 + \sqrt[1.41]{H_3}\,L_3\right] \quad \ldots\ldots\ldots(59a)$$

and,

$$V = \frac{4\,\kappa_b}{D_u^5}\left[\sqrt[1.72]{H_1}\,K_{b1} + \sqrt[1.72]{H_2}\,K_{b2}\right] \quad \ldots\ldots\ldots\ldots(59b)$$

in Equation (58) and solving for B,

$$B = \frac{U}{D^x} + \frac{V}{D^y} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(60)$$

which corresponds to Equation (24) and can be solved in a similar manner to yield:

$$D_u = \sqrt[6.9]{\frac{U + V}{B}} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(61)$$

Since κ_f, H, L, K_s, and K_b, corresponding to Equation (27), are definite constant values, Equation (61) can be solved. It is also advisable for comparison to compute Equation (61) changing the root from 6.9 to 6.0. so that the limits of the error may be judged.

The foregoing development of the basic formula, Equation (27), extends its application to all penstocks in which H is greater than 300 ft.

Discussion of Fig. 1.—In regard to the Q-value to be used in Equation (43) Professor Ludin prescribes the determination of the equivalent discharge:

$$Q' = Q_{\text{Equivalent}} = \sqrt[3]{\frac{\Sigma\, q^3}{\Sigma\, t}} \qquad (62)$$

in which q = partial discharge, in cubic feet per second, during a time interval,[28] t, for every different case.

The authors' computation of different loss-factor values in Fig. 1 is very interesting. It shows that for the usual values of load factor, from $F = 0.4$ to $F = 0.8$, the loss factor ranges from $f = 0.11$ to $f = 0.235$, and, from $f = 0.54$ to $f = 0.685$, respectively, if the curves, E and C, are assumed as limits; or from $f = 0.065$ to $f = 0.5$, and from $f = 0.52$ to

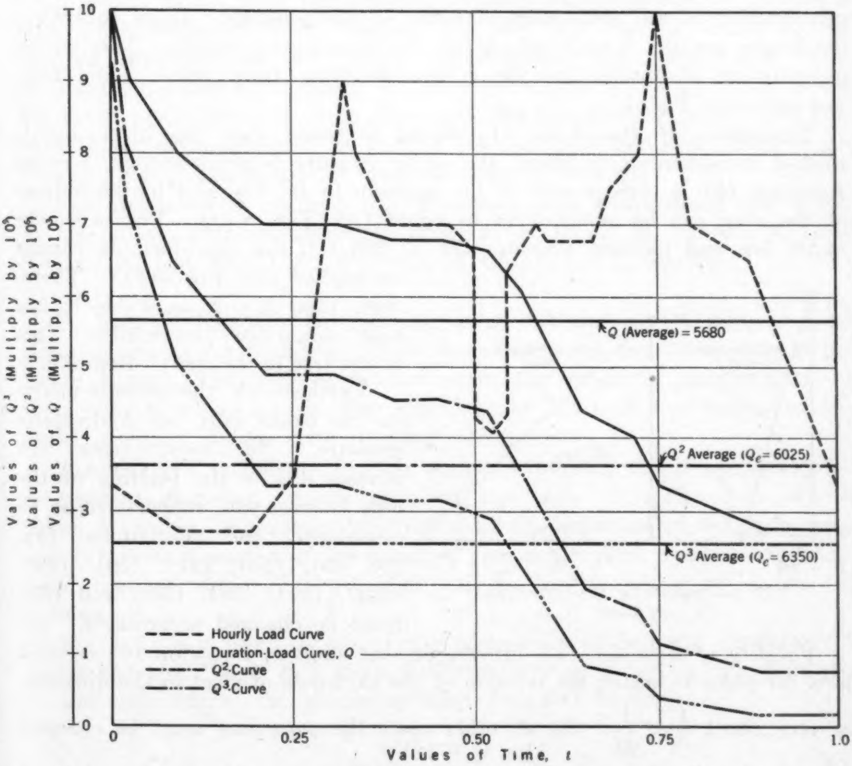

FIG. 4.—COMPUTATION OF EQUIVALENT DISCHARGES

$f = 0.8$, respectively, if Curves A and B are assumed as limits. This proves that the error in determining the penstock diameter for small load factors can be very high. For $f = 0.4$, for example, the error in Curves C

[28] Manuals of Engineering Practice, Am. Soc. C. E., No. 11, p. 6.

and E equals $\sqrt[6.9]{\frac{0.235}{0.110}} - 1 = 0.116$; and in Curves A and B, $\sqrt[6.9]{\frac{0.40}{0.065}} - 1 = 0.3013$. The error is between 11.6% and 30.13%, thus nullifying the value of the meticulous and laborious determination of the economic diameter.

The writer considers that it is impossible to give any general factors, such as a loss factor, or any limiting values for the exact computation of penstocks. Every region or district has its own characteristics which must be considered in each case separately. This can be determined, correctly and quickly, in the following manner.

A load-duration curve is usually computed in any case. For different points on such a curve compute the values of Q^2 and Q^3, as shown in Fig. 4. The second and third roots, respectively, the averages of these values, in Equation (62), will give the correct equivalent values of Q_e for the computation of the economical diameter of the penstock. These equivalent discharges are the correct values for the consumption region. This simple computation eliminates any inexactness resulting from values taken from the curves in Fig. 1.

Limitation of Discussion.—It should be noted that this discussion is limited to penstocks, in which the water pressure is great enough, because Equation (6) is correct only if the pressure in the top and in the bottom of the ring can be assumed to be equal (see Fig. 5(*a*)). For penstocks under low and medium pressure ($H < 200$ ft), the ring formula cannot be applied (see Fig. 5(*b*)). Therefore, Item No. 2, in Table 3, has been computed incorrectly. This is also true in the case of Item No. 3.

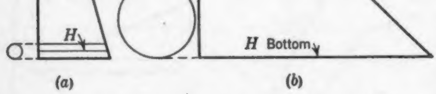

FIG. 5.—PRESSURE DISTRIBUTION IN PIPE

Furthermore, the present analysis is based only on hydrostatic pressure. In most cases the stresses due to the bending of the pipe between the supports must be considered and Equations (6), (7), (9), (10), (11), (20), (22), (23*d*), (27), (28), (30), and (33) must be changed accordingly.

Validity of Equation (6).—In the present analysis Equation (6) is valid only for pipes in which the relation of the thickness of steel to the diameter is very small, $\frac{t}{D} < \frac{1}{20}$. In all other cases the equations must be changed accordingly. Professor Ludin proposes using the outside diameter instead of the inside diameter in Equations (11) and (44).

Other Limiting Factors.—Finally, the diameter is limited due to the fact that, in cases of large discharges, two or more penstock lines are used. This effect can be considered by reducing the equivalent discharge by the number of pipes. Diameters of 21.30 ft and 17.42 ft (Items Nos. 3 and 6,

Table 3) seem to be too large to be advantageous in actual operation. Fig. 2 demonstrates that for small heads and large discharges, the values of the penstock diameters do not change very much. For example, when $H = 100$ ft and $Q = 5\,000$ cu ft per sec, $D = 25.50$ ft; and for the same head and $Q = 6\,000$ cu ft per sec, $D = 26.60$ ft. In other words, a change of 20% in the rate of flow, changes the diameter only 7.6 per cent. This is clear because of the unimportance of friction losses in the computation of large diameters. Furthermore, the factors discussed herein under the heading, "Limitation of Discussion," should be considered as well. Thus, the values given in Items Nos. 3 and 6, Table 3, should be considered as being not exactly correct. The present analysis, therefore, is limited to penstocks of small and medium diameters (not greater than 12.0 ft), and great heights ($H > 300$ ft).

It seems to be very questionable whether the diameters of penstocks situated immediately down stream from the dam can be found by the analysis suggested in this paper. Furthermore, the construction of a nomogram has two advantages: (1) It permits finding the maximum heads for given discharges and given diameters, for the standard sizes; and (2), once it has been computed no further calculation is required. Fig. 2 can replace all the calculations in ordinary cases of penstocks.

For assistance in reading this discussion and encouraging the writer in his efforts, acknowledgment is freely given to R. W. Powell, M. Am. Soc. C. E., and Mr. D. C. Williams, of Ohio State University.

F. Knapp,[29] Esq. (by letter).[29a]—It is a well known fact that, in most cases, penstocks for high-head plants have diameters decreasing toward the power house. The authors do not state the range of applicability of their own formula, nor do they mention the limit of application of the usual orthodox design with varying diameters.

The analysis submitted by the authors, resulting in a penstock with a constant diameter, is applicable as long as: (1) The cost of the penstock per ton is constant irrespective of the variation of the diameters; and (2) the top section of the penstock does not require a minimum thickness, imposed either for practical reasons or in virtue of danger of collapse.

Let ΔL be a short length of the pipe, with an internal diameter, D, under the total head, H. With a circumferential stress, s, the thickness of the shell (using the simple cylinder formula) becomes:

$$t = \frac{0.434\, H\, D \times 12}{2\, s} \quad \text{.......................(63)}$$

Now, assume that the pipe forms a cone with the small angle, $2\,\alpha$, at the summit. Using the symbol, D_a, to denote average diameter, the

[29] Licensed Engr., São Paulo, Brazil.

[29a] Received by the Secretary March 20, 1937.

weight of the pipe with the length, ΔL, is:

$$W = w\, D_a\, \pi \frac{\Delta L\, t}{\cos \alpha \times 12} = \frac{w\, D^2_a\, \pi\, H \times \Delta L \times 0.434 \times 12}{2\, s \cos \alpha \times 12} = \frac{\text{constant}}{\cos \alpha} \quad (64)$$

in which w = the weight of steel, in pounds per cubic foot (= 490).

The loss of head, due to friction, is given by:

$$h_f = K \frac{\Delta L}{D_a} \frac{V^2}{2\, g} = K_1 \frac{Q^2}{D^5_a} \Delta L \quad (65)$$

Angle α does not appear in Equation (65) and this means that the loss of head in the two cases (constant diameter and decreasing diameter) is the same. However, the weight of the pipe, according to Equation (64), becomes a minimum when $\cos \alpha = 1$. The diameter, therefore, should be constant.

The foregoing two assumptions are the basic requirements for the applicability of the authors' formula. However, in cases where the thickness of the top section of the penstock is not determined by stress conditions, or when a special type of pipe is used in the bottom section (such as forged or banded pipe) which has the effect of increasing unit cost sharply, then the diameters of the penstock, to be the most economical ones, should be stepped.

Certain high-head penstocks are designed on the basis of accidental surge conditions. In such cases, the determination of the design head, including water-hammer, needs to include the influence of variations of diameter and thickness.[30]

A paper on high-head penstock design presented in 1933 contains a proposal to increase the diameters toward the power house,[31] in order to avoid the considerably increased surge in the second positive period, which is sometimes as great as 200% of the primary surge, due to reflections at changes of diameters and thicknesses. This proposal, fortunately, has not been adopted, as it is based on an incomplete study. For penstocks with decreasing diameters and which are designed to take extreme surge conditions into account, the writer has shown that the "reflection-times" of the various diameter sections should be exactly equal and, furthermore, that the "transmission-factors" for the surge wave should be nearly equal.[32] These two requirements (actually incorporated in the design of three important high-head penstocks), together with the consideration of the influence of head losses, have the economic advantage of reducing the absolute maximum surge to values either equal to, or only slightly greater than, the primary surge.

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[30] "High Head Penstock Design," by A. W. K. Billings, M. Am. Soc. C. E., O. H. Dodkin, F. Knapp, and A. Santos, Jr., Assoc. M. Am. Soc. C. E., "Symposium on Water-Hammer," Hydraulic Division, A. S. M. E., Power Division, Am. Soc. C. E., 1933.

[31] *Loc. cit.*, p. 44.

[32] "Simple Graphical Solution for Pressure Rise in Pipes and Pump Discharge Lines," by R. W. Angus, *Proceedings*, The Eng. Inst. of Canada, 1935, Vol. 18, p. 72; discussion by F. Knapp, p. 267.

For such a penstock design with limitations, imposed by surge conditions, it becomes almost impossible to determine the best diameters by analytic methods. For the aforementioned penstocks, the writer developed a direct graphical method without the use of "cut and dry" methods, which permits investigating all influencing factors with a negligible amount of labor.

The authors based their analysis on minimum total annual cost. This assumption corresponds to a situation in which funds for investment in the project are limited only by the necessity of making the project as attractive as possible. From an economic point of view, it would be justifiable to go beyond the point of minimum total yearly cost to a point corresponding to the case where funds for investment are unlimited, provided the return on each dollar invested will equal or exceed a fixed minimum.

Most penstocks are designed on the basis of "output." In certain cases, it becomes necessary to base the analysis on "capacity" and according to the prevailing conditions, both output and capacity must be taken into account.

It appears from the foregoing that too much refinement of analytical methods is not warranted, and that all such computations should be used, combined with sound judgment, to arrive at the best solution.

RALPH W. POWELL,[33] M. AM. SOC. C. E. (by letter).[33a]—In their "Synopsis" the authors state that their formulas differ from those heretofore published in three particulars. The writer believes that Items (*b*) and (*c*) constitute worth while improvements, although in the case of Item (*c*) it may be noted that where a hydro-electric plant is operated at low load factor, it is usually to supply "peak power" which would be more valuable, so that the product, $f\,b$, in Equation (11), may be fairly constant after all. However the authors' form would be useful if it were adapted (as it easily could be) to the case of the economic diameter of a pipe through which water is to be pumped. If the use were not continuous, the economic diameter would certainly be somewhat less than for continuous operation at capacity.

H. L. Doolittle,[13] M. Am. Soc. C. E., as well as William P. Creager and Joel D. Justin, Members, Am. Soc. C. E. (who were cited by the authors), brought the load factor into the problem. Mr. Doolittle stated very clearly that the annual cost of the lost power will vary as the average of the cubes of the various flows; he drew a duration curve and another curve obtained by cubing the ordinates of the duration curve; and showed that f is the cube root of the ratio of the average ordinate of the latter curve to its maximum ordinate. It seems that this is the only satisfactory way to obtain the value, f, and that it cannot be obtained from F alone.

[33] Assoc. Prof. of Mechanics, Ohio State Univ., Columbus, Ohio.

[33a] Received by the Secretary March 25, 1937.

[13] "A Method for the Economic Design of Penstocks", by H. L. Doolittle, *Transactions*, A. S. M. E., Vol. 46, 1924 (Paper No. 1946), pp. 1165–1178.

There are many possible duration curves, all giving the same power factor, which will yield quite different values of f. For instance, suppose a straight line varying from maximum to zero, and a curve the ordinates of which vary as one plus the cosine of an angle from 0 to 180 degrees. Both give $F = 0.500$, but the first gives $f = 0.250$ and the second, $f = 0.125$ (if 3 is used as the exponent of Q. If 2.9 is used, as the authors have done, $f = 0.256$ and 0.134, respectively. These correspond to Curves D and B of Fig. 1).[33b]

Returning now to the consideration of Item (a), the writer questions whether the Scobey formula is the best on which to base the theory of economic design, and whether it would not be better to assume that friction losses vary as the square of the velocity. This statement represents a change in opinion, because in 1927 the writer proposed a formula[34] based on the Hazen-Williams formula for pipe friction. This gave an exponent of 0.146 which is equivalent to the 6.85 root in Equation (11) as compared to 6.9 given by the authors, and 7 by the simpler theory now proposed. This was before Mr. Scobey published his formula. Four or five years later, the writer would undoubtedly have followed Mr. Scobey, as the authors have done; but in the last few years additional light has been thrown on frictional losses at high Reynolds' numbers by J. Nikuradse,[35] V. L. Streeter, Jun. Am. Soc. C. E.,[36] H. Schlichting,[37] and others. These data have been summarized in English by Mr. Streeter and Hunter Rouse, Assoc. M. Am. Soc. C. E.[38] Fig. 2 in Mr. Streeter's paper, and Fig. 10 in that of Mr. Rouse, give Nikuradse's results. The lowest curve, representing a roughness of $\dfrac{1}{507}$ of the radius of the pipe, would seem to be suitable for most steel penstocks. At any rate plotting Weisbach's f from Mr. Scobey's original data against the Reynolds' number, it seems to agree with this curve as well as with his own equation. Attention is especially called to Runs 65, 66 and 312 of his paper[4] which show a definite tendency of Weisbach's f to increase in the region at a Reynolds' number of approximately 1 000 000, just as it does in Nikuradse's lower curve. (Of course, various types of riveting, age of pipe, etc., will give a series of curves, some lower and some higher, with their minimum points at somewhat different Reynolds' numbers.)

If Nikuradse is correct (and Streeter and Schlichting have confirmed his work), the correct exponent of the velocity to give the friction head

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[33b] Correction for *Transactions:* In Equation (40), write $f_B = f_C^{1.5}$.

[34] "Economic Diameter of Pipe Lines", *Engineering News-Record,* Vol. 98, March 24, 1927, p. 499.

[35] "Strömungsgesetze in rauhen Röhren", von J. Nikuradse, Verein Deutscher Ingenieur, *Forschungsheft,* No. 361, 1933.

[36] "Frictional Resistance in Artificially Roughened Pipe", by Victor L. Streeter, *Transactions,* Am. Soc. C. E., Vol. 101 (1936), pp. 681–713.

[37] "Experimentelle Untersuchungen zum Rauhigkeitsproblem", von H. Schlichting, *Ingenieur-Archiv,* Band VII, Heft 1, 1936.

[38] "Modern Conceptions of the Mechanics of Fluid Turbulence", by Hunter Rouse, *Proceedings,* Am. Soc. C. E., January, 1936, pp. 21–63.

[4] "The Flow of Water in Riveted Steel and Analogous Pipes", by Fred C. Scobey, M. Am. Soc. C. E., *Technical Bulletin No. 150,* U. S. Dept. of Agriculture, January, 1930.

in a typical large steel pipe would vary from 1.75 for Reynolds' numbers between about 4 000 and 50 000; then it would increase to 2.00 at about 200 000, and to 2.10 at about 300 000; decrease to 2.00 at about 1 000 000; and remain constant for higher values. It would seem, therefore, that for the Reynolds' numbers usually encountered in a penstock, 2 would be as good a value to use as any. This would change Equation (11) to the seventh root, Equations (32) and (33) to the sixth root, and would greatly simplify the intermediate equations.

It should perhaps be re-emphasized that Equations (11) to (27) apply to high heads where the necessary thickness varies directly as the product of the head and diameter; and Equations (32) and (33) to the almost equally important case of low head in which the thickness of the shell is independent of both the head and the diameter. The writer must confess that he has never studied thoroughly the design of large pipe lines,[39] but he raises the question as to whether there is not a third case to be considered—that in which the maximum shear in the pipe shell, considered as a beam, will govern the thickness so that it will be independent of the head but dependent upon the diameter.

The writer does not wish these remarks to be taken as adverse criticism of the paper as a whole, because he considers it a valuable piece of work done in a careful and thorough manner. He is particularly sorry that the senior author cannot participate in the discussion of the points herein raised.[39a] Many years ago he had the privilege of working for some weeks in the same room as Mr. Voetsch and under his immediate supervision. No young engineer could ask for a more kindly chief, or one with wider experience or sounder engineering judgment.

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[39] "Design of Large Pipe Lines", by Herman Schorer, Assoc. M. Am. Soc. C. E., *Transactions*, Am. Soc. C. E., Vol. 98 (1933), pp. 101–191; and "Line Load Action on Thin Cylindrical Shells", by Herman Schorer, *Transactions*, Am. Soc. C. E., Vol. 101 (1936), pp. 767–810.

[39a] Mr. Voetsch died on February 7, 1935.

AMERICAN SOCIETY OF CIVIL ENGINEERS

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DISCUSSIONS

CONSTRUCTION AND TESTING OF HYDRAULIC MODELS MUSKINGUM WATER-SHED PROJECT

Discussion

BY MESSRS. G. W. HOWARD, F. W. EDWARDS, AND T. T. KNAPPEN

G. W. HOWARD,[6] JUN. AM. SOC. C. E. (by letter).[6a]—In view of the increasing use of model studies and the corresponding increase in difficulties of construction and operation of models, this paper is extremely helpful in its presentation of the author's experiences in conducting the studies made in connection with the Muskingum Water-Shed Project.

It is doubtful whether the troubles that are encountered with piezometers can be over-emphasized. The authors state that, of all measurements taken, the piezometer readings were the most difficult and unsatisfactory. This is generally true in almost every study of the type discussed, where much use is made of piezometer readings.[6b] The general difficulty encountered is in fluctuations in the manometer tubes, caused by entrained air. To care for this factor, it is frequently possible to install bleeder valves at certain points in the line to permit correction of this difficulty and also to flush the lead line.

When there are excessive fluctuations in the tube, it is also possible to use several arrangements for dampening these fluctuations in an effort to obtain an average reading. One method is to choke the line, either by using a clamp or by a reduction in size of the line. Another method is to have a stilling-well, say, 9 in. by 9 in. by 15 in., into which the line is led before it is taken to the manometer tube or gage bucket. This second method has been found by the writer to be preferable to choking the line. However, any method for dampening fluctuations to obtain an

NOTE.—The paper by George E. Barnes, M. Am. Soc. C. E., and J. G. Jobes, Jun. Am. Soc. C. E., was published in December, 1936, *Proceedings*. This discussion is printed in *Proceedings* in order that the views expressed may be brought before all members for further discussion of the paper.

[6] Junior Engr., U. S. Waterways Experiment Station, Vicksburg, Miss.

[6a] Received by the Secretary March 11, 1937.

[6b] Correction for *Transactions:* In December, 1936, *Proceedings*, p. 1514, under the heading, "Piezometer Readings", "A piezometer orifice with $\frac{1}{4}$-in. hole and $\frac{1}{16}$-in. radius of rounding is recommended for standard practice by C. M. Allen, M. Am. Soc. C. E. For smaller pipe sizes he suggests a $\frac{1}{8}$-in. hole with $\frac{1}{32}$-in. radius.[5] The latter suggestion was adopted by the writers for model tunnels about 4 in. to 8 in. in diameter."

average reading must be used with caution. Each gage should be investigated carefully to determine the time required for equilibrium to be reached, because inconsistencies in results can quite often be traced to a manometer observation made prior to equilibrium.

F. W. EDWARDS,[7] JUN. AM. SOC C. E. (by letter).[7a]—Former papers on hydraulic investigations utilizing models have described briefly the laboratory equipment and the physical features of the models as incidental to the more detailed presentation of the test results. In this paper, the opposite is true. Two objectives are reached by this change: First, to other research workers, many hints on model construction and operation are given; and, second, to the designers who put into practical use the results of tests, some of the problems to be met and some of the limitations to be expected in model studies are presented, thus promoting better understanding between the research and the design groups.

It is proposed herein to add a few remarks on the economics of model investigations, to elaborate somewhat upon the operation difficulties listed by the authors, and to discuss the relation between the capacity of the prototype and the model as mentioned in the paper.

Economics of Model Investigations.—The cost of a model investigation is difficult to determine in advance. As emphasized by the authors, caution should be used in comparing the costs of different models. The construction cost for some types of models is relatively large whereas the operation cost of others is the major item. The cost of the construction of the first test set-up and of the operation of the first series of proposed tests may be estimated readily, but the results of the first few tests may change the nature of the entire future program. Before the completion of the study, it is often necessary to develop new instruments to obtain the results desired. Furthermore, models similar in character studied at one place during an extensive program (as were the Muskingum studies), will cost less on the average than a single model.

Offsetting the uncertainty of cost of a proposed investigation is the fact that model studies usually point to economies either by indicating inadequate designs, by directly reducing construction costs, or by both. Savings created by the elimination of unsafe designs are indirect and often cannot be evaluated accurately. However, there are many examples of reductions in construction cost, subject to accurate estimation, which were created entirely by model investigations.

An example of a model study which eliminated an unsafe design and, at the same time, reduced the estimated cost of the structure is that of the stilling-basin for the Conchas Dam which was completed at the U. S. Waterways Experiment Station, at Vicksburg, Miss. This study exposed a dangerous flow condition in the basin for the preliminary plan and led to the adoption of an alternate plan having a reported cost[8] of approxi-

[7] Associate Engr., U. S. Engr. Office, 2d New Orleans Dist., New Orleans, La.

[7a] Received by the Secretary, April 5, 1937.

[8] "Hydraulic Laboratory Projects of the Corps of Engineers, U. S. Army," by F. H. Falkner, Jun. Am. Soc. C. E., *Transactions*, A. S. M. E., Vol. 58, No. 7, p. 561.

mately $625 000 less than the original design. The cost of the model study was less than $10 000.

The estimated construction cost reductions which were made possible by the tests for the Muskingum Water-Shed Project are not known to the writer, but the general statement made by the authors, that "* * * the model studies resulted usually in reducing the length or width of the basins, or both," indicates that the money saved made the cost of the investigation appear insignificant. There are many other examples of great economies resulting from model experimentation.

Operation Difficulties.—Operation difficulties place definite limitations upon model studies. The recognition of these limitations is essential to proper analysis of the test results. The authors state that piezometer readings are the most difficult to obtain accurately. Of course, the importance of the various measurements depends upon the particular problem being studied, but the writer believes that, ordinarily, velocity measurements in the type of model dealt with in the paper are the most difficult and unsatisfactory. Pitot tubes, Bentzel tubes, and small current meters all have their shortcomings particularly in exceptionally turbulent flow similar to that encountered in a stilling-basin. Sometimes it is possible to obtain relative values, but in some cases it is practically impossible to obtain velocity measurements that have any value whatever. New instruments based on different principles are needed for these measurements.

Entrained air, referred to in the paper, is not only a factor in scale conversions, but often becomes a cource of trouble in pizeometer and Pitot tube connections. These connections, from orifice opening to manometer, should be as short as possible. In addition to avoiding sharp bends, they should slope upward throughout their length to eliminate sags where air often becomes entrapped. Mechanical devices for drawing the air out of connections are desirable if not essential. Vacuum pumps or air ejectors are most satisfactory for this purpose.

Capacity of Prototype and Model.—The authors mention the impossibility of reproducing the frictional factor to scale in models of the size of those discussed. They suggest measuring the value of n experimentally and then using it to determine the discharge conversion factor. In a model in which the roughness is significant, this method must be used with caution because the corresponding stages may be considerably in error and the location of the hydraulic jump affected accordingly. For example, consider a model that has too great a value for n. For normal head-water elevation, the conduit discharge would be too small. Under this circumstance a tail-water elevation in the model corresponding to the correct prototype discharge would cause the hydraulic jump to form up stream from its correct position. Particularly in a basin having warped sides and sloping floor, this condition would be difficult to analyze. However, by varying the tail-water elevation for a given discharge, sufficient data may be collected to permit proper analysis of the basin action when the head-water elevation, conduit discharge, and tail-water elevation are correlated.

One solution for the problem arising from the inability to reduce the friction factor below a certain limit is to use a model that is large enough to eliminate the scale effect. This often would require space and discharge capacity beyond the laboratory facilities.

Another solution, which would eliminate the conduit section and provide for studying the intake and stilling-basin in separate models, should be considered for each specific problem. The friction loss in the conduit section would be left entirely to computation.

T. T. KNAPPEN,[9] M. AM. SOC. C. E. (by letter)[9a].—The general plan for the Muskingum project, referred to by the authors as the "Official Plan", was prepared by the U. S. Engineer Office, at Zanesville, Ohio, in co-operation with the Muskingum Watershed Conservancy District and was completed about August, 1934. This project then was constructed by the U. S. Engineer Department under the co-operative agreement between the Muskingum Watershed Conservancy District and the U. S. Engineers, representing the Public Works Administration. Since the work was to be constructed with P.W.A. funds and since the law at that time required that all funds be obligated by July 1, 1935, the Designing Staff was faced with the problem of completing the plans and specifications for the entire project by the following May, in order to allow a sufficient period for advertising the work and awarding the contracts. In analyzing the design, it was apparent that numerous difficult hydraulic problems would develop and that the time for their solution would be limited. Negotiations were started, therefore, early in the summer of 1934 with the object of devising an arrangement for testing the proposed hydraulic designs in a suitably equipped hydraulic laboratory. An agreement was finally made with Professor Barnes to have this work done at the Case School of Applied Science, in Cleveland, Ohio, and Mr. Jobes was assigned as representative of the U. S. Engineer Office to assist Professor Barnes in performing the experiments and to serve as a "contact man" between the two organizations.

At the time that these negotiations were going forward, a section was being organized in the Zanesville Office to design the hydraulic structures. Mr. Paul H. Jaenichen was placed in charge of this section. The preliminary hydraulic designs were developed under his direction, and laboratory tests were than run to check and improve the tentative plans. A close relation was maintained at all times between the design section and the laboratory. The hydraulic model experiments were considered as being a part of the actual design. Throughout the course of the experiments, this policy was strictly maintained. Under ordinary circumstances one might have been warranted in conducting these experiments to extend the range of test data beyond the immediate needs of a specific design problem and to obtain information of more general applicability; but the severe time limitation made it necessary to dismantle each model and replace it with another one as soon as the particular problem was sufficiently clarified.

[9] Principal Engr., North Atlantic Div., U. S. Engrs., New York, N. Y.

[9a] Received by the Secretary April 12, 1937.

Exceptions were made in cases where existing models could be used with little or no alteration for preliminary investigations of similar features on other dams in the project.

Table 1, showing the cost of labor and materials involved in the model testing, is known to be accurate, as a strict system of accounting was established as a basis of approving bills for the work. The over-head charges of 40% are reasonably representative of the actual over-head costs. The total cost of the hydraulic model work, including over-head, was approximately $42 000, or an average cost of about $4 000 per model. The construction cost of the fourteen reservoirs of the Muskingum project was approximately $23 000 000, so that the cost of the hydraulic model testing was slightly less than 0.2% of the project cost. Although hydraulic experiments were made for only eleven of the dams, the results were used on all fourteen. The writer feels that the expenditure was more than justified by the results obtained.

At the beginning of the hydraulic design work, little information was available about hydraulic-jump action in sloping and flaring channels. Considerable difficulties were encountered in the first designs, and many changes were necessary in the first models, in order to arrive at a good stilling-basin arrangement; but as the hydraulic experimental work progressed and test results from the first models were analyzed, definite design rules were developed and the art of designing stilling-basins was gradually mastered. Two models of the outlet structure of the Wills Creek twin conduit were built before a satisfactory solution was found; but the largest part of this cost should be distributed among the other dams, since all the later designs benefited by these first experiments. The designs of the Mohawk and Bolivar outlet works, also twin-conduit structures, gave such surprisingly good check results during the model tests that it was found unnecessary to perform experiments on the Beach City structure, which is in principle the same as the Wills Creek layout. The cost of the Mohawk tests was relatively high, due to the completeness with which the intake structure and the conduit system were reproduced in the model.

The experience with the small single-conduit outlets was similar to that with the large twin-conduit structures. The Tappen and Clendening outlets were treated very completely in the experimental work, and numerous changes were made in these models. The stilling-basin designs for Piedmont and Pleasant Hill were based on these experimental findings, and when tested in the models gave such close check on the original designs that it was felt that the Leesville and Atwood structures could safely be eliminated from the experimental program. The relatively high cost figures for the hydraulic experimental work for the Charles Mill, Dover, Mohicansville, and Pleasant Hill structures are due to the fact that the spillways were included in these models and that the design problems were of an intricate nature.

The use of the hump in the stilling-basin apron at the ends of the tunnels for Tappan, Clendening, and Piedmont, first developed by Mr. R. G. Schneider while assisting in the design of these structures, proved

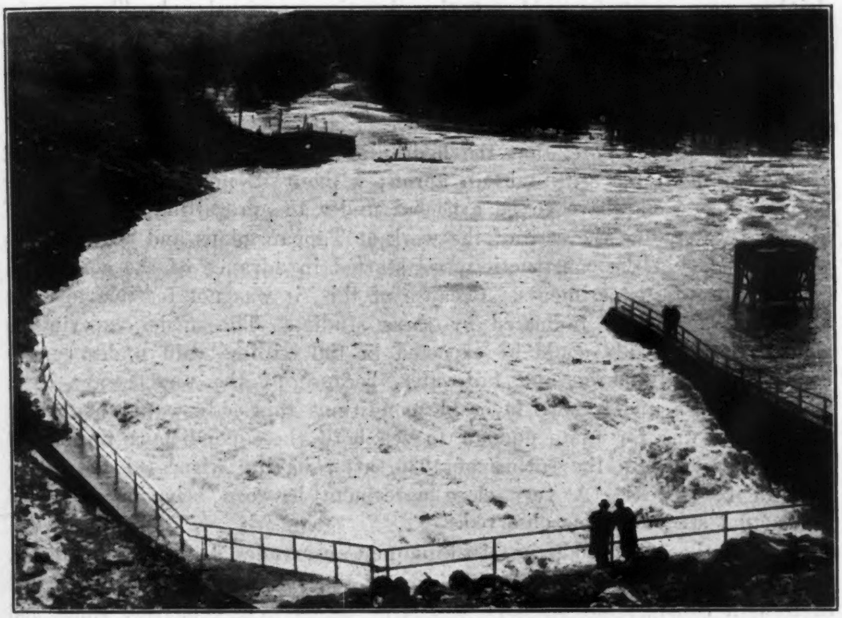

FIG. 11.—WILLS CREEK DAM, OHIO, 1937: STILLING-BASIN FROM TOP OF DAM (DISCHARGE, 15 000 CUBIC FEET PER SECOND)

FIG. 12.—MOHAWK DAM, OHIO, 1937: STILLING-BASIN (DISCHARGE, 16 000 CUBIC FEET PER SECOND)

to be an interesting feature. In these cases the conditions for tunnel driving were most favorable at the elevations selected. At Tappan, for instance, if the tunnel had been raised to the required invert elevation to avoid the use of the hump, the crown would have broken through a hard sandstone into soft shale which would have required timbering the tunnel at an increased cost of about $10 000.

It is worthy of note, that the hydraulic jump at Clendening is created by the hump in the stilling-basin apron; without it, unsatisfactory stilling performance would have to be expected under the prevailing conditions of high tail-water. In the case of the work at Tappan, plans and specifications were issued, and the construction was started in advance of the completion of the hydraulic experiments. Because of this, it was not feasible to make all the modifications indicated by these studies. The model experiments showed that an eddy could be expected in the stilling-basin under certain conditions of discharge and tail-water, because of the unnecessary basin width, whereas under more favorable conditions this eddy would not occur. The writer was fortunate enough to see both these conditions reproduced in the prototype in the spring of 1936, after having witnessed the condition in the model. A very close agreement between behavior of model and actual structure was observed.

During the 1937 flood, the Muskingum Conservancy District was unable, because of legal difficulties, to close any of the gates for regulation, so that even with considerable outflows, discharge heads were relatively small and tail-water levels were higher than normal, due to the small controlling effect of the reservoirs. Thus, stilling-basins during this flood were subject to an extreme and very unusual test. In all cases observed, the hydraulic jump developed very close to the exit portal. The condition of outflow with low kinetic energy and high tail-water is difficult of proper treatment in the design, due to the proximity of unbalanced hydraulic-jump conditions, but the structures acted remarkably well under this test. In comparison with the hydraulic model work, it may be stated herein that the appearance of the stilling performance in the actual structures was far better than that found under similar conditions in the model tests. Various small and rather unimportant flow details that had been observed on the models were reproduced in the actual structures in striking similarity.

An interesting point developed by the model tests was that the flare of the side-walls and the width of the stilling-basin should be limited to obtain the best hydraulic-jump action. This was borne out by a comparison of the action of the Wills Creek and Mohawk Dams during the 1937 flood (Figs. 11 and 12). In their general layout these outlet structures have much in common, both having large twin conduits. The length of the Wills Creek Basin was limited by the dropping off of the rock formation under the far end of the right-basin wall. Its sloping apron is slightly too steep, and the basin width required to reduce the exit velocity to safe limits is somewhat too large for proper jump action. Fig. 11 shows the Wills

Creek Dam discharging about 15 000 cu ft per sec, or 7 500 cu ft per sec from each conduit, during the January, 1937, flood. The back rolls along each side-wall, induced by the departure from ideal conditions described previously, are plainly visible. Fig. 12 shows the Mohawk Dam during the same flood, discharging about 16 000 cu ft per sec under similar tail-water conditions. The attainment of close to an ideal design is indicated by the absence of back rolls.

The writer would like to emphasize the effect of baffle-blocks as described by the authors. The model studies showed repeatedly that properly located baffle-blocks stabilize the jump action, permit the use of somewhat shorter stilling-basins, and provide a factor of safety against unforeseen low tail-water conditions.

A controlling influence in the design of outlet structures is the correct determination of the tail-water conditions. The writer was impressed, by the Muskingum model studies, with the importance of securing accurate survey data from which to compute rating curves below dam sites.

The authors are to be congratulated on presenting this excellent paper, and particularly for setting forth cost data and describing in considerable detail the method of model construction. As they suggest, the cost data must be used with caution, as they are representative of a project involving a large number of structures. Had the model works not been grouped in a continuous program, the costs would have been probably 25% to 50% greater. The writer feels that the expenditure was more than justified and believes that the engineering budget for a similar project should include an allowance for model tests as a part of the necessary and warranted design cost.

The writer concludes from his experience on the Muskingum project that model tests of this type should not be considered as a full substitute for careful analytical design. In combining, properly, hydraulic model experiments with theoretical methods, feasible solution of hydraulic problems may be obtained at a minimum engineering cost. The hydraulic engineer should use the model technique where the laws of hydraulics and hydrodynamics are insufficiently known or their application excessively complicated. The hydraulic problems affecting the design of reservoir outlet work are so involved that it is only by the most careful use of all available methods of attack that acceptable results can be obtained.

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DISCUSSIONS

DEFLECTIONS BY GEOMETRY

Discussion

BY WILLIAM BERTWELL, ASSOC. M. AM. SOC. C. E.

WILLIAM BERTWELL,[9] ASSOC. M. AM. SOC. C. E. (by letter).[9a]—Being simple and explicit, this valuable paper is easy to follow. An actual trial of the geometric method should convince most engineers that the usual textbook treatment of many problems (particularly arch analysis) is cumbersome and difficult to remember. The author is to be congratulated for his careful development of the fundamental conceptions as well as for his choice of illustrations.

The virtues of the geometric attack are especially apparent in dealing with trusses, as Mr. Hall shows. Although it is equally convenient to compute the deflections of flexural sections by geometry, in this case the geometric method is also, essentially, the method of elastic weights, since the elastic weight of an element of an elastic rib may be defined as the angular distortion caused by the application of a unit moment couple to that element. Unfortunately, however, the method of elastic weights has come to be completely identified with the conjugate beam convention in the minds of many engineers, and, hence, it may be that the adoption of such a term as "geometric method" is desirable.

For some time the writer has been using the double summation procedure for the calculation of influence lines for the redundants of a fixed arch, and has found this process to be simpler and shorter than any other, unless one uses a definite mathematical arch such as that developed by Charles S. Whitney, M. Am. Soc. C. E.[10] For designers who take rib-shortening into account in arch design, it might be explained that this effect may be included quite simply in the geometric method. For example, in Table 1, the elastic shortenings of the elements are to be computed and the components of these shortenings in the directions of

NOTE.—The paper by David B. Hall, Assoc. M. Am. Soc. C. E., was published in December, 1936, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: February, 1937, by Messrs, A. J. McCaw, and L. E. Grinter; and April, 1937, by Ralph W. Stewart, M. Am. Soc. C. E.

[9] Associate Civ. Engr., U. S. Bureau of Public Roads, San Francisco, Calif.

[9a] Received by the Secretary March 17, 1937.

[10] *Transactions*, Am. Soc. C. E., Vol. 88 (1925), p. 931.

X-X and *Y-Y* recorded.[10a] Then a single summation of the vertical components will provide the corrections to the vertical deflections at the division points, and the total of the horizontal components gives the correction to the horizontal movement of the hinge point.

The author's analysis of the unsymmetrical beam of Fig. 10(*c*) is brief, but the results may be reached with equal facility by adhering to the general method—the mathematical equivalent of the mechanical method. In this case, the analysis may be made by removing one support, fixing the other, and then applying a unit moment and a vertical force at the free end such that the total deflection of that end is zero. The problem is easily visualized in this manner, and one needs no formulas for the evaluation of the constants of the moment-distribution method; it is only necessary to remember their definitions. This process is exemplified in Table 6 by an analysis for one end of the unsymmetrical beam used by the author. (In Table 6, in addition to the notation of the paper, $\Delta = \frac{3.52}{t^3}$; and y = deflection.)

The author's division of the beam into twelve full sections and two half-sections draws attention to a common inaccuracy which can be easily eliminated. Briefly, the correct influence values, found by any process of finite summation, occur only at the divisions between the finite elements—not at their centers. The writer realizes that it is unnecessary to secure extremely accurate results because of the designer's many assumptions, such as the magnitude of the live load and impact forces, the degree of elasticity of reinforced concrete members, and the effect of reinforcing steel in such members. Nevertheless, it is apparent that the results sought are the angular distortion of the element and the deflection of one end from the tangent at the other. The first is the product of the moment and the elastic weight, and the second is the product of the angular distortion and the distance from the instantaneous center of rotation to the face of the element. Hence, one can calculate true deflections only at the division points between the elements. In the graphical solution this means that the true influence curve is tangent to the equilibrium polygon at the division points between the beam elements. Column (11) of Table 6 takes account of this, and it is apparent that the extra labor is slight.

In Column (13), Table 6, are values obtained from an analytical solution of the same beam, taking into account all refinements except shear distortion and the curvature of the top and bottom of the beam between division points. If these values are considered comparatively exact, the error of the writer's approximate solution is only about one-half as great as that which results from using the points at the centers of the elements. The error of the author's solution is small, however, and this may be attributed to his having used rather short divisions of the beam.

[10a] Corrections to *Transactions:* In Equation (15) change "$\phi_A B$" to "ϕ_{AB}".

TABLE 6—Influence Line for Fixed-Beam Moments at End A

Division point	Depth of beam, t	$\frac{3.52}{t^3} = \Delta$	Unit Moment at A: $\Sigma\Delta = \theta$	Unit Moment at A: $\Sigma\theta = \frac{y}{3.52}$	Unit Vertical Force at A: $\frac{x}{3.52}$	Unit Vertical Force at A: $\frac{x\Delta}{3.52}$	Unit Vertical Force at A: $\frac{\Sigma(x\Delta)}{3.52} = \frac{\theta}{3.52}$	Unit Vertical Force at A: $\Sigma\frac{\theta}{3.52} = \frac{y}{3.52^2}$	(3.52 V = 0.138907) $\frac{y}{3.52}$	($M = 1$; and $V = \frac{0.138907}{3.52}$) $\frac{y}{3.52}$	($M = 1$; and $V = \frac{0.138907}{3.52}$) $\frac{y}{3.52}$ = mean values from Column (10)	Influence ordinates $= \frac{y}{0.7388}$	True values
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
A				54.8603				394.9423	54.8603	0		0	0
	4.23	0.0465	8.3593	50.6807	0.5	0.0232	54.8602	367.5122	51.0500	0.3693			
1											0.7171	3.416	3.425
	3.315	0.0965	8.3128	42.3679	1.5	0.1447	54.8370	312.6752	43.4328	1.0649			
2											1.3745	6.548	6.572
	2.555	0.2110	8.2163	34.1516	2.5	0.5275	54.6923	257.9829	35.8356	1.6840			
3											1.9248	9.170	9.214
	1.99	0.4467	8.0053	26.1463	3.5	1.5634	54.1648	203.8181	28.3118	2.1655			
4											2.2915	10.917	10.988
	1.58	0.8924	7.5586	18.5877	4.5	4.0158	52.6014	151.2167	21.0051	2.4174			
5											2.3761	11.320	11.417
	1.325	1.5132	6.6662	11.9215	5.5	8.3226	48.5856	102.6311	14.2562	2.3347			
6											2.1148	10.075	10.156
	1.255	1.7808	5.1530	6.7685	6.5	11.5752	40.2630	62.3681	8.6634	1.8949			
7											1.5885	7.568	7.581
	1.33	1.4962	3.3722	3.3963	7.5	11.2215	28.6878	33.6803	4.6784	1.2821			
8											1.0070	4.798	4.762
	1.535	0.9732	1.8760	1.5203	8.5	8.2722	17.4663	16.2140	2.2522	0.7319			
9											0.5448	2.596	2.552
	1.915	0.5012	0.9028	0.6175	9.5	4.7614	9.1941	7.0199	0.9751	0.3576			
10											0.2506	1.194	1.163
	2.455	0.2379	0.4016	0.2159	10.5	2.4980	4.4327	2.5872	0.3594	0.1435			
11											0.0910	0.434	0.416
	3.16	0.1115	0.1637	0.0522	11.5	1.2822	1.9347	0.6525	0.0906	0.0384			
12											0.0192	0.091	0.084
	4.07	0.0522	0.0522	0	12.5	0.6525	0.6525	0	0	0			
B												0	0

	Influence ordinates (12)	True values (13)
$3.52\ V = \frac{54.8603}{394.9423} = 0.138907$; or V = ...	0.039462	0.039482
Total angular rotation at A = 8.3593 − 0.138907 × 54.8602 = ...	0.7388	0.7458
K_A = reciprocal of total angular rotation at A = ...	1.3535	1.3408
Carry-over factor = 1 − 13 × 0.138907 = ...	0.80579	0.80670
K_{AB} = 0.80579 × 1.3535 = ...	1.0906	1.0817

It may be well to explain the refinements used in obtaining the "exact" values in Column (13), Table 6. Consider the fundamental elastic properties of a trapezoidal element. It may be shown that the center of gravity of infinitesimal elastic weights is at the intersection of the diagonals, rather than at the mid-point between the parallel bases. Furthermore, the true elastic weight of the element is measured by the sum of the two parallel bases divided by the square of their product, rather than by the reciprocal of the cube of the depth at the mid-point. For example, if the larger base is 1.25 times the smaller, and the computations are made in the ordinary manner, the error in the location of the elastic center of the element is 5.55% of the length of the element, and the error in the value of the elastic weight is 2.46 per cent. The ratio of 1.25 between the bases is not uncommon, and is slightly exceeded in the author's example. The problem is still further complicated because the instantaneous center of rotation developed in each element by the vertical force at the end is not at the center of gravity of the infinitesimal elastic weights, but at the center of gravity of the infinitesimal angle changes caused by the force. This shift in the center of rotation, when a shear causes the moment in the element (that is, when the moment is not constant throughout the element), is not usually large except for the one or two elements adjacent to the point where the vertical force is applied. In the writer's solution of the author's example, this shift is 0.51 ft for Element *A*-1 and 0.18 ft for Element 1-2. It is a simple matter to use the correct elastic weights, but the variations in their location would indicate that a graphical solution, in which the equilibrium polygon corresponds to the elastic line of the beam, would provide greater accuracy with less labor than the analytical solution. However, an analytical solution taking all these refinements into account is not too lengthy if a computing machine is available. In fact, in this instance only two more operations would have been necessary to include the effect of the shearing distortion.

AMERICAN SOCIETY OF CIVIL ENGINEERS

Founded November 5, 1852

DISCUSSIONS

GRAPHICAL DISTRIBUTION OF VERTICAL PRESSURE BENEATH FOUNDATIONS

Discussion

BY MESSRS. WILLIAM P. KIMBALL, I. M. NELIDOV, GEORGE PAASWELL, AND JACOB FELD

WILLIAM P. KIMBALL,[7] ASSOC. M. AM. SOC. C. E. (by letter).[7a]—As long as the Boussinesq equation is used for the determination of vertical stresses in soil underlying foundations, accurate short-cut methods of applying the equation to actual problems will be of value to the soil engineer. In his paper the author has provided such a tool. The writer has had many occasions to compute, by the Boussinesq equation, the stresses under foundations of various types, and after experimenting with the author's graphical method believes that it represents one of the most efficient time-saving devices available for the solution of most stress-distribution problems.

To illustrate the application of the method the author has shown the analysis of stresses under the centers of individual spread footings supporting a building. A different type of foundation to which the method is equally applicable is a single large area, such as the base of a bridge pier. In this case it is not enough to find the stresses at various elevations under only the center of the area. The net settlement of a bridge pier due to the consolidation of the underlying soil must be caused by the average stresses under the loaded area rather than by the maximum stresses which occur, at least at a distance below the loaded area, under the center of the area. The determination of the average stresses at various elevations under the loaded area becomes necessary. Using the pressure chart for $z = 10$ ft (Fig. 3) the writer has determined the stresses under various points of a footing, 20 by 20 ft in area, uniformly loaded with 1 ton per sq ft. These stresses, together with the resulting stress contours, are shown in Fig. 6. From these contours it is a simple matter to compute, with satisfactory accuracy, the average stress over the entire area. In this particular instance the average stress was com-

NOTE.—The paper by Donald M. Burmister, Assoc. M. Am. Soc. C. E., was published in January, 1937, *Proceedings*. This discussion is printed in *Proceedings* in order that the views expressed may be brought before all members for further discussion of the paper.

[7] Asst. Prof. of Civ. Eng., Thayer School of Civ. Eng., Dartmouth Coll., Hanover, N. H.

[7a] Received by the Secretary February 18, 1937.

puted to be 0.500 ton per sq ft, as compared with the maximum center stress of 0.696 ton per sq ft. The difference between the stress under the center and the average stress decreases, of course, as the distance below the loaded area increases, and the limit beyond which it is not necessary to compute the average stress can be determined readily by practice.

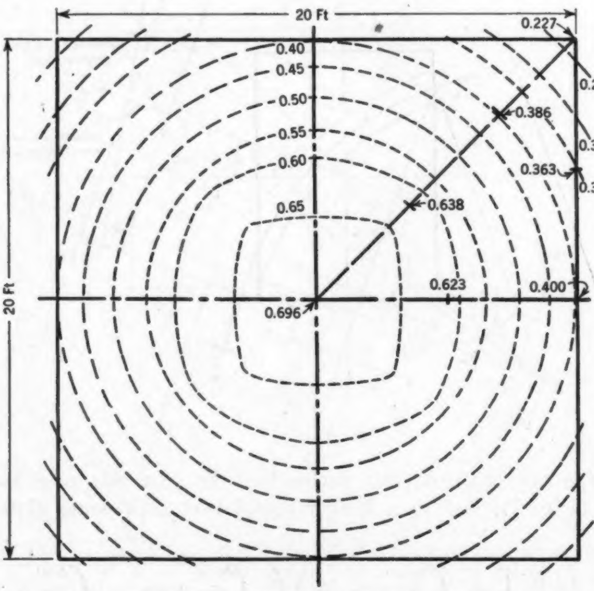

FIG. 6.—VERTICAL PRESSURES AT $z=10$ FEET BENEATH A 20-FOOT BY 20-FOOT LOADED AREA

An additional short-cut which may be used in certain cases has been previously suggested[8] by the writer, namely, to compute the stress under the center as under the center of a uniformly loaded equivalent circular area. An equivalent circular area to the 20 by 20-ft square has a diameter of 22.5 ft. Substituting the proper values in Equation (3) of the paper a center stress of 0.707 ton per sq ft is obtained for $z = 10$ ft. This compares favorably with the 0.696 ton per sq ft obtained by the graphical method, and results in a substantial saving of time. The accuracy of this approximation increases, quite apparently, as the distance below the loaded area increases, but decreases as the shape of the loaded area digresses from the circular form. Whether this approximation is sufficiently accurate in any particular case can only be determined from experience.

I. M. NELIDOV,[9] ASSOC. M. AM. SOC. C. E. (by letter).[9a]—Without raising questions as to the distribution of pressure in the upper strata of foundations, the value of the concentration factor, n, and other factors

[8] *Proceedings* Am. Soc. C. E., August, 1933, p. 1060.

[9] Senior Engr. of Hydr. Structure Design, State Dept. of Public Works, Sacramento, Calif.

[9a] Received by the Secretary March 4, 1937.

pertinent to the problem of the behavior of soils under load, the writer wishes to apply the principle proposed by the author to the case in which

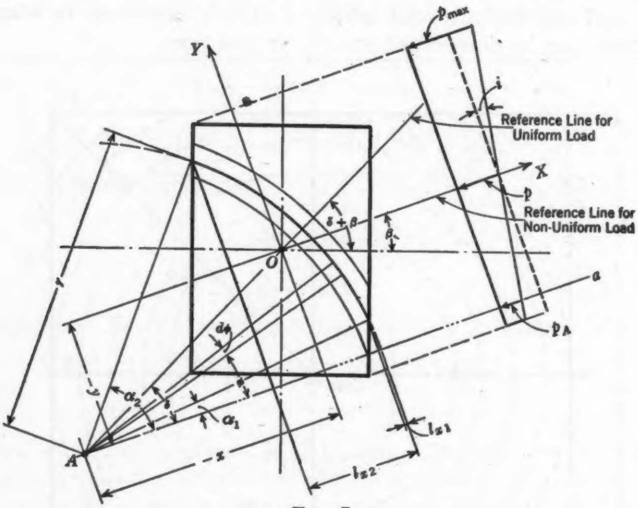

FIG. 7.

the pressure is not uniform, but varies linearly. In this case the pressure at Point A (Fig. 7), due to a loaded foundation area some distance away, will be:

$$p_z = \sum \left\{ \frac{\Delta K'}{2\pi} \left[p_A \int_{\phi=\alpha_1}^{\phi=\alpha_2} d\phi + r \tan i \left(\int_{\phi=0}^{\phi=\alpha^2} \sin\phi \, d\phi - \int_{\phi=0}^{\phi=\alpha^1} \sin\phi \, d\phi \right) \right] \right\}$$

$$= \sum \left[p_A \frac{\Delta K'}{2\pi} (\alpha_2 - \alpha_1) + \tan i \frac{\Delta K'}{2\pi} \left(l_{x2} - l_{x1} \right) \right] = p_{z1} + p_{z2} \ldots (6)$$

in which, in addition to the notation of the paper: p_A is the uniform pressure at Point A, in tons per square foot; p_{z1} and p_{z2} are correspondingly the pressure due to uniform and non-uniform loading; $\tan i$ is the slope of the linear variation of the stress taken normally to the line of zero stress due to bending only, in tons per cubic foot (see Fig. 7); and l_{x1} and l_{x2} are the projection of the arc of radius, r, intercepted by the loaded area, on the line, O-X.

The derivations for obtaining the pressure, p_{z1}, are given by the author. To determine the pressure, p_{z2}, Fig. 8 is constructed similarly to Fig. 3. This is done by taking as abscissas the radii, r, and laying out the distances, l_x, to the left of the point of intersection of the arc corresponding to r, with the line of abscissas, and erecting perpendiculars to the intersection with the arc. The distances, l_x, are selected so as to give even intervals of the load, p_{z2}, with $\tan i = 1$. In this manner,

$$l_x = p_{z2} \frac{2\pi}{\Delta K'} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (7)$$

in which p_{z2} is the load, in tons per square foot. For small values of the loads the intersection was determined analytically from the expression, $\cos \Delta = 1 \frac{l_z}{r}$ in which Δ is the angle between r and the reference line. In order to compute the pressure, p_{z2}, the loaded area is placed on the

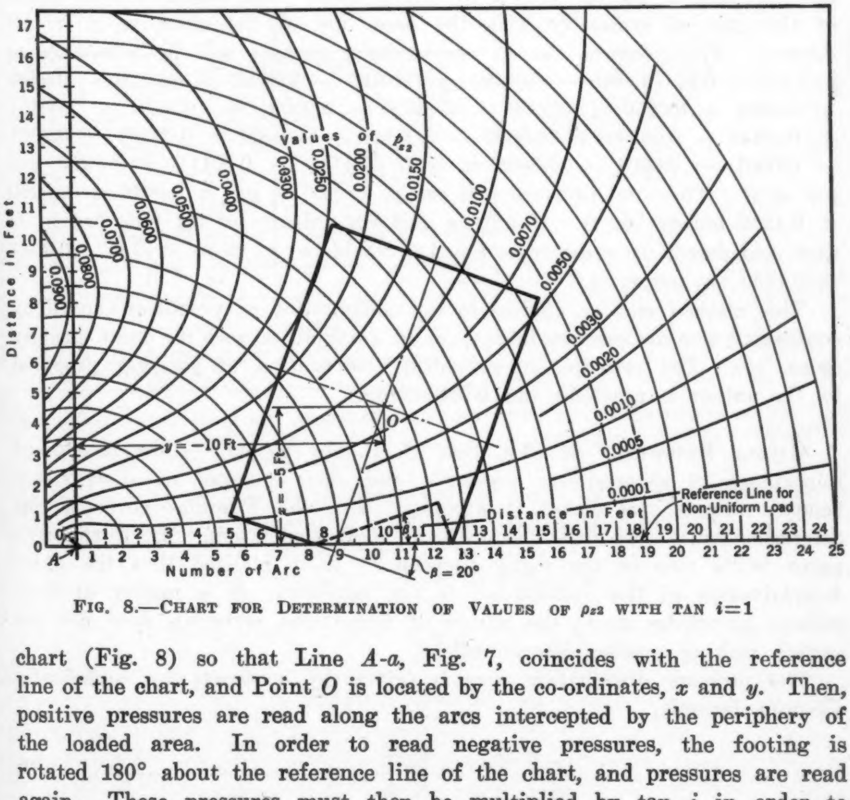

FIG. 8.—CHART FOR DETERMINATION OF VALUES OF p_{z2} WITH TAN $i=1$

chart (Fig. 8) so that Line *A-a*, Fig. 7, coincides with the reference line of the chart, and Point *O* is located by the co-ordinates, x and y. Then, positive pressures are read along the arcs intercepted by the periphery of the loaded area. In order to read negative pressures, the footing is rotated 180° about the reference line of the chart, and pressures are read again. These pressures must then be multiplied by tan i in order to obtain true values of p_{z2}.

For example, consider a rectangular footing 7 ft by 10 ft in area. The uniform pressure is 2.5 tons per sq ft; tan i = 0.15; and the line of zero bending stress is inclined to the axis of symmetry of the footing at an angle, $\beta = 20$ degrees. It is required to find the pressures, p_z, at a point, *A*, with the co-ordinates, $x = -10$ ft and $y = -5$ ft in the co-ordinate system, *X O Y* (Fig. 7).

The intensity of the uniform pressure at Point *A* is: $p = 2.5 \times 0.15 \times 5 = 1.75$ tons per sq ft; the total pressure at Point *A* due to this uniform pressure is obtained with the aid of Fig. 3; and, the angle of inclination of the axis of symmetry of the footing to the base line of the chart, $\beta + \delta$ equals $20 + 27 = 47$ degrees. Consequently, by Equa-

tion (6), $p_{z1} = 1.75$ (0.0015 + 0.0025 + 0.0028 + 0.0030 + 0.0029 + 0.0027 + 0.0024 + 0.0022 + 0.0012 + 0.0006 + 0.0003) + (0.0033 + 0.0045 + 0.0048 + 0.0047 + 0.0044 + 0.0041 + 0.0033 + 0.0016 + 0.0008 + 0.0004 + 0.0001) = 1.75 × 0.0541 = 0.0946 ton per sq ft.

The compressed part of the footing is shown in Fig. 8 in full lines, and the tension part is shown in dotted lines. The angle of inclination of the axis of symmetry with the base line of the chart is $\beta = 20$ degrees. The pressure due to non-uniform stresses will be, similarly to p_{z1}: $p_{z2} = 0.15$ (0.0040 − 0.0010) + (0.0100 − 0.0002) + 0.0160 + 0.0205 + 0.0230 + 0.0250 + 0.0250 + (0.0250 − 0.0002) + (0.0230 − 0.0015) + (0.0145 − 0.0029) + (0.0095 − 0.0042) + (0.0066 − 0.0050) − 0.0001 − 0.0001 − 0.0004 − 0.0005 = 0.15 (0.1741 − 0.0011) = 0.0260 ton per sq ft. The total pressure will be: $p_z = p_{z1} + p_{z2} = 0.0946 + 0.0260 = 0.1206$ ton per sq ft. If only a uniform pressure of 2.5 tons per sq ft were considered, the pressure at Point *A* would be: $p_z = p_{z1} = 2.5 \times 0.0541 = 0.1350$ ton per sq ft.

This method may be applied to the computation of pressures caused by continuous non-uniform loadings, such as earth-fill or rock-fill dams, gravity dams, etc. The principle of graphical computation of pressure proposed by the author is practical and labor-saving.

George Paaswell,[10] M. Am. Soc. C. E. (by letter).[10a]—The recognized importance of determining pressures and their manner of distribution beneath foundations makes it necessary that the formulas covering the determination of such distributions be of ready application. Mr. Burmister's paper is "a step in the right direction." It is realized that the exact determination of the distribution is not necessary; as a matter of fact, present knowledge as to the nature of foundation materials does not yet permit making precise determinations.

The pressure distribution may be given by a simple yet sufficiently accurate formula,

$$r = \frac{0.32}{\sqrt{k}} \qquad (8)$$

in which k is the ratio of the vertical pressure intensity at a given point to the total applied surface load, and r is the radius of the circle along which the pressure intensity bears the constant ratio, k, to the total surface load. A family of spherical surfaces is given by Equation (8), all tangent at the point of loading. Fig. 9 gives the comparison of such loci, Fig. 9(*a*) being plotted by the Boussinesq equation, and Fig. 9 (*b*) by Equation (8).

The question of the partition of a concentrated load into sufficiently small areas to give correct distribution becomes important at points close to the load. Professor A. E. H. Love has given a solution[11] for the

[10] Spencer & Ross, Inc., Detroit, Mich.

[10a] Received by the Secretary March 15, 1937.

[11] *Philosophical Transactions* Royal Soc., Series A, 1929.

distribution of pressures beneath a rectangle, Fig. 10. The vertical intensity of pressure at a point, x, y, z, with respect to the center of the rectangle as the origin is,

$$V = -\frac{A - z\,B}{2\,\pi}\,p \quad \text{.........................(9)}$$

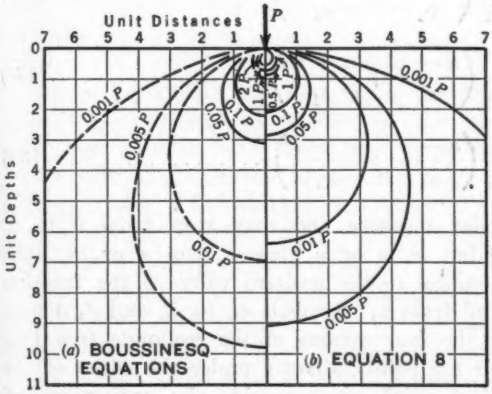

FIG. 9.—LOCI OF CONSTANT VERTICAL PRESSURE INTENSITIES.

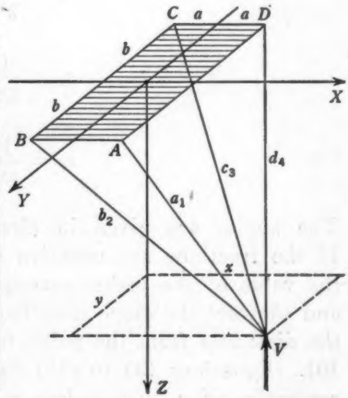

FIG. 10.—DISTRIBUTION OF VERTICAL PRESSURES UNDER UNIFORMLY LOADED RECTANGLE.

in which p is the surface load per unit of area. The other terms are as follows (a negative value of V denotes a compressive stress):

$$A = -\left[2\,\pi - (\phi_1 + \phi_2 + \phi_3 + \phi_4)\right] \quad \text{..............(10a)}$$

$$B = (a - x)\,s_1 + (a + x)\,s_2 + (b - y)\,s_3 + (b + y)\,s_4 \quad \text{......(10b)}$$

$$\cos\phi_1 = \frac{(a - x)\,(b - y)}{k_1\,k_3} \quad \text{..................(11a)}$$

$$\cos\phi_2 = \frac{(a - x)\,(b + y)}{k_1\,k_4} \quad \text{..................(11b)}$$

$$\cos\phi_3 = \frac{(a + x)\,(b - y)}{k_2\,k_3} \quad \text{..................(11c)}$$

$$\cos\phi_4 = \frac{(a + x)\,(b + y)}{k_2\,k_4} \quad \text{..................(11d)}$$

$$k^2_1 = (a - x)^2 + z^2 \quad \text{......................(12a)}$$

$$k^2_3 = (b - y)^2 + z^2 \quad \text{......................(12b)}$$

$$k^2_2 = (a + x)^2 + z^2 \quad \text{......................(12c)}$$

$$k^2_4 = (b + y)^2 + z^2 \quad \text{.....................} (12d)$$

$$s_1 = \frac{1}{k^2_1}\left(\frac{b - y}{a_1} + \frac{b + y}{d_4}\right) \quad \text{..................} (13a)$$

$$s_2 = \frac{1}{k^2_2}\left(\frac{b - y}{b_2} + \frac{b + y}{c_3}\right) \quad \text{..................} (13b)$$

$$s_3 = \frac{1}{k^2_3}\left(\frac{a - x}{a_1} + \frac{a + x}{b_2}\right) \quad \text{..................} (13c)$$

$$s_4 = \frac{1}{k^2_4}\left(\frac{a - x}{d_4} + \frac{a + x}{c_3}\right) \quad \text{..................} (13d)$$

The angles are given in circular measure and may vary from 0 to π. If the fractions are negative (that is, x or y greater than a or b), take the value of the cosine corresponding to the positive value of the fraction and subtract the angle thus found from π. Symbols a_1, b_2, c_3, and d_4 denote the distances from the point to the four corners of the rectangle (see Fig. 10). Equations (9) to (13) fair for points directly under the edges of the rectangle. For such points select points just outside the projections of the rectangle.

It may be interesting to observe how many subdivisions are required, using the Boussinesq formula, to approximate the values as obtained from Equation (9). Select a series of points directly below the center of the rectangle, $x = y = 0$, and assume that $a = 5$; $b = 3$; and $p = 1$. The area of the rectangle is 60 units, and the total load, P, in the Boussinesq formula, is 60.

TABLE 5.—COMPARISON OF VERTICAL PRESSURE INTENSITIES

Values of z	Love	Boussinesq		
		One subdivision	Four subdivisions	Eight subdivisions
1	0.98	28.6	0.01	0.29
2	0.91	7.2	0.42	0.73
3	0.79	3.2	0.61	0.78
4	0.66	1.8	0.62	0.68
5	0.54	1.1	0.60	0.56
10	0.23	0.3	0.23	0.23
20	0.06	0.07	0.07	0.07

Table 5 shows the vertical intensity of the pressure at depths from 1 to 20 as given by Love and by Boussinesq when the rectangle is subdivided into one, four, and eight parts It indicates that a single concentration may be assumed for the rectangular distribution when the point in question is at least 10 ft below the surface.

The writer feels that the method of plane sections[4] as covered in the

[4] Progress Rept., Special Committee on Earths and Foundations, *Proceedings*, Am. Soc. C. E., May, 1933, p. 789.

report of the Committee on Earths and Foundations, is an extremely useful one in considering the effect of a group of pier loads on the deeper strata. A uniform distribution of load may be taken as the equivalent of the various piers and the simple formulas given in the report may be used to determine the vertical loadings at the depths required. The prediction of settlement from an analysis made in this manner will be exact enough in every sense, to make a complete study of the foundation problem.

JACOB FELD,[12] M. AM. SOC. C. E. (by letter).[12a]—The graphical interpretation, by Professor Burmister, of the integration of pressure distributions over volumes below the loaded areas is a time-saving tool, which does not reduce the accuracy of the method. However, it is important that one who uses it knows the limitations of the theory and does not expect a result more accurate than can be expected from a tedious analytical computation based on that theory.

Very properly, the author distinguishes between the problems of "disturbed zone" and deeper layers as an explanation of settlements. The Boussinesq formula, with or without modifications, is only applicable in isotropic bodies. The soils in a "disturbed zone" directly below a foundation are not isotropic. The soils in deeper layers, under initial compression (before the additional loading) are often under such low stresses, that isotropy is closely simulated. Hence, the formula and the author's method are not applicable to the determination of settlements in the "disturbed zone," but can be used for such determinations at greater depths.

The careful reader will notice that the theoretical load distribution is not dependent upon the soil characteristics. This is neither logical nor true. Under low stresses, the error introduced by the assumption that all soils are equal, is so quantitatively small, because the numerical values are small, that it can be disregarded.

Especially in the "disturbed zone" any theory that is based on the assumption that, under equal loading the same distribution will result in mud and in rock, must require modification.

Referring to the numerical example, especially Table 4, and from a general study of the problem of settlements under foundations, one is quite safe in drawing the following conclusions:

(*a*) At depths of 20 ft, or more, the assumption of a single point loading for the usual column footings gives pressure distributions (computed either numerically or graphically) which are as accurate as the computed loadings on such footings.

(*b*) At lesser depths, if the soil is of such nature that isotropic behavior cannot be expected, settlements will occur which cannot be permitted. In this case, loads are transmitted to lower and better layers of soil, by means of piles or caissons.

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[12] Cons. Engr., New York, N. Y.

[12a] Received by the Secretary April 15, 1937.

(*c*) At lesser depths, if the soil is of such nature that isotropic behavior can be expected, settlements are so small that there is no problem.

In all these studies for the estimation of expected settlements, in addition to the nature of the soil, the designer often disregards the effect of the shape of the footings and edge resistance, as well as relative settlements of unequal areas loaded with the same load per unit area. These effects are most important in soils that cannot be assumed to fall in the isotropic group.

AMERICAN SOCIETY OF CIVIL ENGINEERS

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DISCUSSIONS

STRUCTURAL ANALYSIS BASED UPON PRINCIPLES PERTAINING TO UNLOADED MODELS

Discussion

By Messrs. James R. Griffith, and Camillo Weiss

James R. Griffith,[23] M. Am. Soc. C. E. (by letter).[23a]—A valuable contribution to the subject of unloaded models is contained in this paper. The principles involved in the determination of influence lines by unit displacements of a model have been somewhat difficult for some engineers to accept. This is particularly true of the influence lines for moment. The mathematical proof in Appendices I and II alone is worthy of consideration. As a mathematical means of computing the numerical value of moment, shear, or thrust at any section of a structure independent of values obtained at any other section, the method is of additional note. When the loads are few and stationary, such a mathematical solution might be more economical in some cases than a solution by model.

In the design of the catenary structures for the terminal electrification of the Illinois Central System, at Chicago, Ill., the writer was one of the first users of the unloaded model. Since that time he has never lost his enthusiasm for the method. As a means of helping students to visualize the action of an indeterminate structure, the method is far superior to any other known to the writer. It has had one serious objection, however, in that the cost of commercial sets for utilizing such models has been prohibitive for the average individual and the smaller offices. The writer has long experimented with various materials and equipment, with cost and availability as the principal objectives.

Anders Bull, M. Am. Soc. C. E., made a real contribution when he described the use of both loaded and unloaded models fabricated of "brass drill rod."[24] The writer has been unable as yet to locate any local supply of this material. Bronze welding rod, of the various materials tried, has

Note.—The paper by Otto Gottschalk, Esq., was published in January, 1937, *Proceedings*. Discussion on the paper has appeared in *Proceedings*, as follows: March, 1937, by Messrs. L. J. Mensch, and Frederick Shapiro.

[23] Prof. of Structural Eng., Oregon State Agri. Coll., Corvallis, Ore.

[23a] Received by the Secretary March 1, 1937.

[24] *Engineering News-Record*, December 8, 1927, p. 920.

thus far best fulfilled the qualifications. It is available in almost every shop equipped to do gas-welding, and the cost is scarcely worth mentioning. Scraps of tin, push pins, and a welding iron are the only other equipment needed.

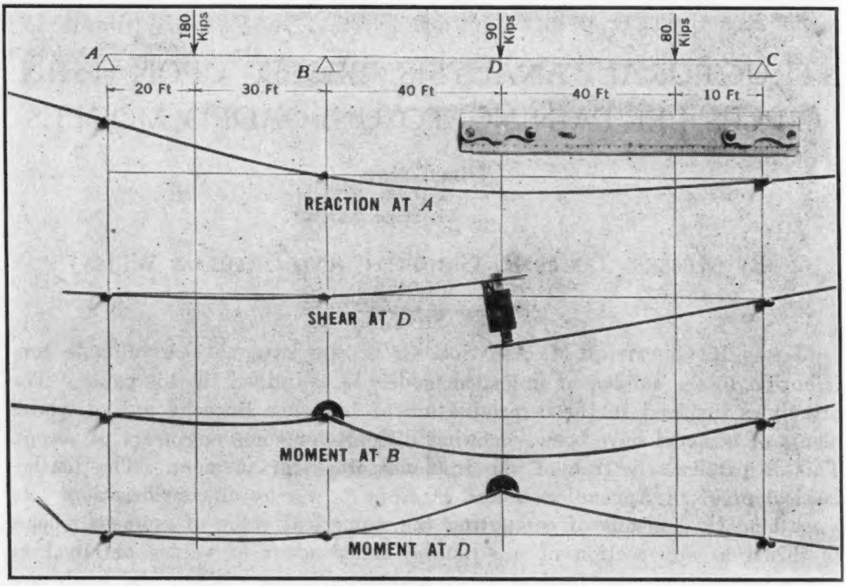

FIG. 20

Fig. 20 shows a series of influence lines constructed by welding wire for the beam of constant section shown. The shear clamp is the development suggested by Ray de Lancey, Jun. Am. Soc. C. E. It is adjustable within a reasonable range of accuracy and is fabricated entirely of tin. The necessary rotation for moment at interior sections is obtained by a tin clamp as shown, which, as in the case of the sheer clamp, can be removed quickly from the wire. Since it was found impossible to fabricate the moment clamps to a predetermined angle within the required accuracy, it was necessary to calibrate them after fabrication.

Theoretically, as the author indicates, the deformation of a model must be very small to avoid error. Practically, even with relatively large deformations, the writer feels that results are obtained well within the limits warranted by the present knowledge of reactions, elastic properties of the finished structure, and rigidity of connections.

CAMILLO WEISS,[25] M. AM. SOC. C. E. (by letter).[25a]—Apart from its usefulness, this is an extremely interesting and stimulating paper. By

[25] Structural Engr., Eng. Dept., Fabricated Steel Constr., Bethlehem Steel Co., Bethlehem, Pa.

[25a] Received by the Secretary March 24, 1937.

virtue of Appendices I and II, this method may be considered as an ingenious combination of Maxwell's theorem with the slope deflection theory, and it may be permissible to compare it with the latter. Such a comparison is also helpful in appraising the merits of the proposed method and to show under what circumstances it is particularly useful.

The author was wise in selecting simple problems to illustrate his method; but because he did so he has not really done his method full justice. Furthermore, the method has certain drawbacks which are not apparent in simple problems.

In Conclusion (3) the author states: "The stress at any section may be computed independently without knowing the moment, shear, etc., at any other section." This is indeed a great advantage. On the other hand, the word, "may", in this Conclusion could be written "must"; and this fact may be a disadvantage at times.

In general, the proposed method will prove most valuable when the loads are movable; when stresses are wanted at only a few points; and when the number of loaded members is comparatively small. For example,

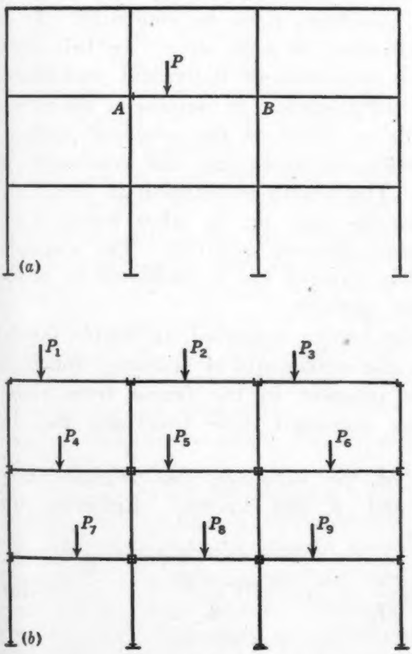

FIG. 21.

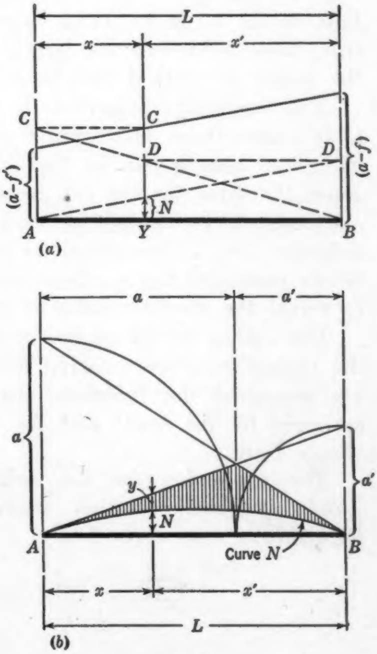

FIG. 22.

in Fig. 21(*a*) all joints are assumed to be free to rotate but are fixed against translation. Suppose that only the moment, M_{AB}, at End A of Member AB, is desired. To obtain the answer by the slope deflection method, twelve unknown slopes and forty-two unknown moments must

be introduced and twelve simultaneous equations must be solved. To obtain the answer by the proposed method, two unknown tangential intercepts and the ordinate of one influence line are the only variables. These three unknown quantities are obtainable from three equations, two of them independent of each other. If Load P is moving along Member $A\,B$, one new ordinate of the influence line is all that is required for each position.

The slope deflection method would necessitate solving a set of twelve simultaneous equations for each position of the load. Therefore, this problem is obviously one for which the proposed method may well be considered superior.

Fig. 21(*b*) represents the same frame, except that in this case every panel is loaded and the moments at the end of every member (forty-two moments in all) are wanted. To obtain the answer by the slope deflection method again involves fifty-four unknowns and requires the solution of twelve simultaneous equations. To obtain the answer by the proposed method eighteen tangential intercepts and nine ordinates of influence lines (twenty-seven variables), are required for each moment; a total, therefore, of $27 \times 42 = 1\,134$ unknown quantities must be computed. It is true, these unknowns are largely independent of each other. In this case, the choice of method will be primarily a matter of individual preference.

The foregoing comparison is admittedly crude. It implies at least two tacit assumptions, both erring probably in favor of the proposed method. The first assumption is that the work of computing the constants is about the same for the two methods. The second assumption is that only one ordinate is wanted for each influence line; or, in other words, each influence line is considered as only one unknown quantity. The example herein presented has not been selected as typical, but is presented in order to reveal the characteristics of the two methods.

The author should be commended for having expressed, in simple terms, the mutual relations existing between the coefficients of stiffness; for having separated the functions that are inherent in the frame from those governed by the load; and for having expressed these functions, too, in simple form.

The expression for the ordinate of the influence line admits of a simple graphical solution when f and f' are known. Referring to Fig. 22(*a*):

$$\overline{AC'} = \overline{YC} = (a - f)\,\frac{x}{L} + (a' - f')\,\frac{x'}{L} \quad \ldots\ldots\ldots\ldots (61a)$$

$$\overline{BD'} = \overline{YD} = \overline{AC'}\,\frac{x'}{L} \quad \ldots\ldots\ldots\ldots\ldots\ldots (61b)$$

and,

$$N = \overline{BD'}\,\frac{x}{L} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (61c)$$

Substituting Equations 61(*a*) and 61(*b*) in Equation 61(*c*), the result is expressed by the author as Equation (18). To construct any point on the curve of this formula, lay off $(a - f)$ above End *B*, Fig. 22(*a*), and $(a' - f')$ above End *A*. The closing line intersects the vertical, *Y*, at Point *C*. Projecting Point *C* horizontally to Point *C'*, Line *C' B* intersects the vertical through Point *Y* at Point *D*. Projecting *D* to *D'*, Line *D' A* intercepts the required ordinate, *N*, on the vertical, *Y*. Proof of the construction can be obtained from the similarity of triangles. In this manner, Curve *N* of Fig. 22(*b*) can be constructed. The remainder of the construction of the influence line in Fig. 22(*b*) is self-explanatory.

Similar construction will furnish the ordinates of the elastic curve, Equation (5), if the intercept *f*, is substituted for $(a - f)$ and f' for $(a' - f')$ in Fig. 22(*a*) and if proper consideration is given to the signs of *f* and f'.

Appendix II.—Referring to Appendix II, Equations (30) are stated to be the formulas used in the slope deflection method, but "the system of signs is different from that generally used." The signs are not in agreement with the "Notation", nor is it stated, what convention has been adopted. The enlightening derivation of the fundamental formulas, as presented in Appendix II, is an essential part of the paper and the absence of a sign convention may be somewhat confusing. Perhaps the following comments will be of service.

Assume that all slopes, deflections, and moments are absolute numerical quantities. As in arithmetic, a plus sign means "add" and a minus sign means "subtract." Fig. 23(*a*) represents a restrained but unloaded member, *A B*. Suppose, it is desired to produce a contraclockwise restraining moment, M_{AB}, at End A. Apart from temperature stresses, this can be done in any of four (and only four)

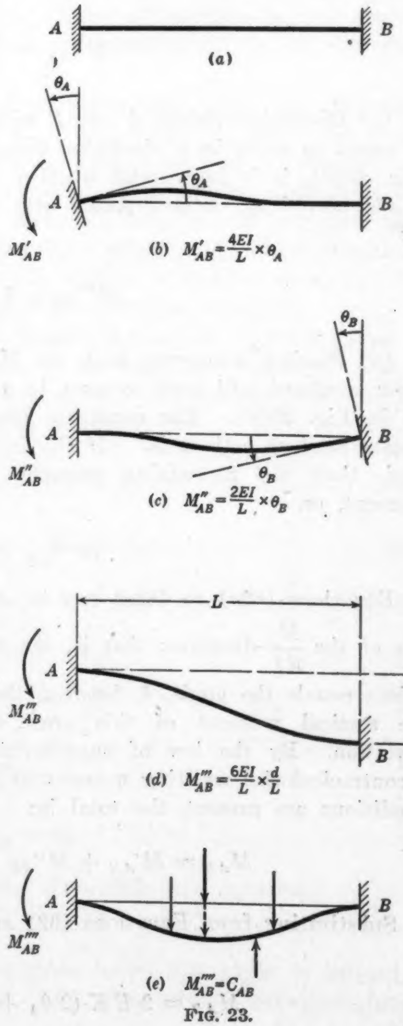

FIG. 23.

different ways illustrated in Figs. 23(*b*), 23(*c*), 23(*d*), and 23(*e*), and in any combination of these four ways.

A contraclockwise restraining moment at A can be obtained by:

(*a*) Rotating End A counterclockwise through an angle, θ_A, as in Fig. 23(*b*). Quantitatively, this moment is:

$$M'_{AB} = \frac{4\,E\,I\,\theta_A}{L} \qquad (62a)$$

(*b*) Rotating End B counterclockwise through an angle, θ_B, as in Fig. 23(*c*). Quantitatively, this moment is:

$$M''_{AB} = \frac{2\,E\,I\,\theta_B}{L} \qquad (62b)$$

(*c*) Displacing Points A and B with respect to each other so that Point B seems to move in a clockwise direction around Point A. Referring to Fig. 23(*d*), it is immaterial whether Point B was moved down or Point A was moved up, or both points were moved. Quantitatively, this moment is:

$$M'''_{AB} = 6\,E\,\frac{I}{L}\,\frac{d}{L} \qquad (62c)$$

(*d*) Placing transverse loads on Member $A\,B$, so that the moment of their resultant will tend to turn in a clockwise direction about Point A, as in Fig. 23(*e*). The condition illustrated in Fig. 23(*e*) is termed a "beam fixed at both ends." If C_{AB} is the end moment at A for the given load, then the restraining moment is numerically equal to this end moment, or,

$$M''''_{AB} = C_{AB} \qquad (62d)$$

Equations (62*a*) to (62*c*) can be derived from two well-known properties of the $\frac{M}{EI}$-diagram; that is, the principles that the area between two points equals the angle, θ, between the tangents at these points and that the statical moment of this area equals the corresponding tangential deviation. By the law of superposition these four methods of creating a contraclockwise resisting moment at A are additive, therefore, if all four conditions are present, the total is:

$$M_{AB} = M'_{AB} + M''_{AB} + M'''_{AB} + M''''_{AB} \qquad (63)$$

Substituting from Equations (62) and writing K for $\frac{I}{L}$ and R for $\frac{d}{L}$:

$$M_{AB} = 2\,E\,K\,(2\,\theta_A + \theta_B + 3\,R) + C_{AB} \qquad (64)$$

Obviously, if any of the four methods of inducing a contraclockwise restraining moment at A acts in a direction opposite to the direction shown in Figs. 23(b) to 23(e), the corresponding moment must be subtracted instead of being added. For instance, if End B is rotated clockwise, instead of contraclockwise, and if there are no transverse loads:

$$M_{AB} = 2\,E\,K\,(2\,\theta_A - \theta_B + 3\,R) \qquad (65)$$

Equation (65) corresponds to the condition shown in Fig. 12, as expressed by Equation (30a), and indicates that the foregoing method of treating slopes and deflections as purely numerical quantities, is apparently the convention adopted in Appendix II. This convention is well suited to the author's proposed method, when deformations of known direction are applied.

It might be mentioned that this convention, although admirably adapted to the purposes of the author, is not convenient when the slopes and deflections are unknown in direction. This is why, in the slope deflection method, it is usual to assume that all rotations and deflections are in the same direction (clockwise), and only the known constant, C_{AB}, is treated as an absolute numerical quantity. When, after computation, certain slopes and deflections are found to be negative quantities, it means that their directions are in reality opposite to the direction assumed.

In effect, Equation (64) is the slope deflection equation, without the use of a sign convention. Introducing the convention that clockwise rotation is to be indicated by a "plus" sign, and contraclockwise rotation by a "minus" sign, and referring again to Figs. 23(b) to 23(e):

$$- M'_{AB} = -4\,E\,K\,\theta_A \qquad (66a)$$

$$- M''_{AB} = -2\,E\,K\,\theta_B \qquad (66b)$$

$$- M'''_{AB} = +6\,E\,K\,R \qquad (66c)$$

and,

$$- M''''_{AB} = +\,C_{AB} \qquad (66d)$$

Adding Equations (66), $- M_{AB} = - M'_{AB} - M''_{AB} - M'''_{AB} - M''''_{AB}$; $- M_{AB} = -2\,E\,K\,(2\,\theta_A + \theta_B - 3\,R) + C_{AB}$; or,

$$M_{AB} = 2\,E\,K\,(2\,\theta_A + \theta_B - 3\,R) - C_{AB} \qquad (67)$$

which is the conventional form of the slope deflection equation.

The following corrections will be made before the paper is printed in *Transactions:* On page 16 January, 1937, *Proceedings,* definition for N,

change "Equation (21)" to read "Equation (18)"; in Line 2, below Fig. 4, change "$S' = 0.75\,k$" to read "$S'_1 = 0.75\,k$"; in Equation (16*a*), change "S'" to "S_B"; in Equation (16*b*) change "S" to "S_A"; in Fig. 5(*a*), the length, $\overline{AE}$, is *a*, not *a'*; in Line 6 below Fig. 16, write: "Since $\frac{\theta_{AB}}{\theta_{BA}} = m'_1$; $\frac{\theta_{CB}}{\theta_{BC}} = m_2$; $\left(1 - \frac{m'_1}{2}\right) k_1 = S'_1$; and $\left(1 - \frac{m_2}{2}\right) k_2 = S_2$:"; in Equation (37*a*) write "S'_1" for "S_1"; and in Equation (37*b*), write: "θ_{BC}" instead of "θ_{BC2}."

AMERICAN SOCIETY OF CIVIL ENGINEERS

Founded November 5, 1852

DISCUSSIONS

STRESSES AROUND CIRCULAR HOLES IN DAMS AND BUTTRESSES

Discussion

By C. P. Vetter, M. Am. Soc. C. E.

C. P. Vetter,[32] M. Am. Soc. C. E. (by letter).[32a]—The analysis of stresses around a circular gallery in a dam, presented in this paper, follows a method of approach somewhat different from that of previous investigations of the same subject. Such further investigations are of considerable interest since they tend to clarify the problem in the minds of students and designers. The main value, however, lies in comparing results with that of previous work, in emphasizing differences and similarities, in indicating improvement, if any, and in comparing the differences in numerical values obtained by the different methods Any such comparison necessitates giving references to previous work, and such references are often the most valuable part of investigations of this nature. The author has given few such references and has made no such comparisons. The subject has been dealt with in detail by Mr. J. H. A. Brahtz,[33] and it is, therefore, worth investigating to what extent Mr. Silverman has added to present knowledge of the subject.

The paper (see Fig. 1) traces the stresses in a section through a circular hole in a bar subject to uniform tension and refers to the work of Professor S. Timoshenko.[4] In the same book Professor Timoshenko gives[34] the complete solution for stresses at any point of the bar outside the circular opening as well as the proper reference to this well-known Kirsch solution.[35]

Note.—The paper by I. K. Silverman, Jun, Am. Soc. C. E., was published in November, 1936, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: February, 1937, by Messrs. R. D. Mindlin, and Chesley J. Posey; and April, 1937, by Messrs. J. H. A. Brahtz, V. L. Fedorov, and F. W. Hanna.

[32] Engr., U. S. Bureau of Reclamation, Denver, Colo.

[32a] Received by the Secretary, March 29, 1937.

[33] "Stresses and Reinforced Steel Around Circular Openings in Concrete Dams," etc., *Technical Memorandum No. 457*, U. S. Bureau of Reclamation, and other memoranda of that Bureau.

[4] "Theory of Elasticity," by S. Timoshenko, p. 78.

[34] *Loc. cit.*, p. 77.

[35] *Zeitschrift*, V. D. I., Vol. 42, 1898.

In the author's notation:

$$s_\rho = \frac{s_x}{2}\left(1 - \frac{r^2_h}{\rho^2}\right) + \frac{s_x}{2}\left(1 + \frac{3\,r^4_h}{\rho^4} - \frac{4\,r^2_h}{\rho^2}\right)\cos 2\,\theta \quad \ldots\text{(104}a\text{)}$$

$$s_\theta = \frac{s_x}{2}\left(1 + \frac{r^2_h}{\rho^2}\right) - \frac{s_x}{2}\left(1 + \frac{3\,r^4_h}{\rho^4}\right)\cos 2\,\theta \quad \ldots\ldots\text{(104}b\text{)}$$

and,

$$s_s = -\frac{s_x}{2}\left(1 - \frac{3\,r^4_h}{\rho^4} + \frac{2\,r^2_h}{\rho^2}\right)\sin 2\,\theta \quad \ldots\ldots\text{(104}c\text{)}$$

The tension, s_x, in the bar is herein assumed as acting along the X-axis and θ is assumed as positive in the usual way from the X-axis toward the Y-axis. If, in addition, the bar is subjected to a tension, s_y, along the Y-axis, the total stress in any point may be obtained by substituting $\theta + \frac{\pi}{2}$ for θ in Equations (104) and adding; thus:

$$s_\rho = \frac{s_x + s_y}{2}\left(1 - \frac{r^2_h}{\rho^2}\right) + \frac{s_x - s_y}{2}\left(1 + \frac{3\,r^4_h}{\rho^4} - \frac{4\,r^2_h}{\rho^2}\right)\cos 2\,\theta \quad ..\text{(105}a\text{)}$$

$$s_\theta = \frac{s_x + s_y}{2}\left(1 + \frac{r^2_h}{\rho^2}\right) - \frac{s_x - s_y}{2}\left(1 + \frac{3\,r^4_h}{\rho^4}\right)\cos 2\,\theta \quad \ldots\text{(105}b\text{)}$$

and,

$$s_s = -\frac{s_x - s_y}{2}\left(1 - \frac{3\,r^4_h}{\rho^4} + \frac{2\,r^2_h}{\rho^2}\right)\sin 2\,\theta \quad \ldots\ldots\text{(105}c\text{)}$$

This solution has previously been given by Mr. Brahtz.

If the co-ordinate axes are chosen along the lines of principal stresses, s_1 and s_2, at the location of the center of the hole, Equations (105) will give the stresses at any point around the hole. Along the periphery ($\rho = r_h$), for example:

$$s_\rho = 0 \quad \ldots\ldots\text{(106}a\text{)}$$

$$s_\theta = s_1 + s_2 - 2\,(s_1 - s_2)\cos 2\,\theta_1 \quad \ldots\ldots\text{(106}b\text{)}$$

and,

$$s_s = 0 \quad \ldots\ldots\text{(106}c\text{)}$$

in which θ_1 now is measured from s_1. The symbols, s_1 and s_2, denote the principal stresses in a solid dam at a point corresponding to the center of the hole in the structure under consideration. The assumptions on which Equations (105) and (106) are based are obviously that the hole is very small, as compared with the other dimensions, and is sufficiently far removed from the boundaries of the dam.

The author seems to have avoided the necessity for the former assumption and his Equations (42) to (46), inclusive, apparently only assume that

the hole is far away from the boundaries of the dam, but not necessarily that it is very small. However, in order to bring his equations to a form which may be of practical use, the author assumes (see following Table 2) that $q = \frac{r_i}{r_o} = 0$. It should be noted that in the main body of his paper, Mr. Silverman does not state that this assumption lies at the foundation of Equations (15) to (17), inclusive. The assumption that $\frac{r_i}{r_o} = 0$ obviously puts the author's equations on par with Equations (105) and (106), obtained from Kirsch's solution, in that they are all based on the same assumptions. Any discussion of the effect of mass, aside from the effect on the principal stresses in the dam, would appear to be a wasted effort since the weight of the concrete in the very small hole cannot possibly affect the stresses to any marked degree.

That the author has gained very little may, perhaps, best be shown by comparing the numerical values obtained by him with those obtained by using Equations (106). At the point on a solid buttress corresponding to the center of the hole in the author's example, the principal stresses are easily obtained[96] from Equations (8) and (22):

$$s_1 = +\,0.0427\; p\, h \;\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (107a)$$

and,

$$s_2 = -\,0.5481\; p\, h \;\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (107b)$$

The angle of the maximum principal stress with the author's X-axis is: $\epsilon = -\,7°\ 38'$; hence; $\theta_1 = \theta - \epsilon = \theta + 7°\ 38'$. From Equations (106) the

TABLE 3.—Comparative Values Obtained from Equations (106)
(In all cases, values to be multiplied by $p\,h$)

θ	s_θ by author	s_θ by Equations (106)	θ	s_θ by author	s_θ by Equations (106)
0°	−1.6825	−1.6453	180°	−1.5877	−1.6453
30°	−0.8094	−0.8059	210°	−0.7976	−0.8059
60°	+0.3583	+0.3340	240°	+0.2937	+0.3340
82°22′	+0.6753	+0.6762*	262°22′	+0.6567	+0.6762*
90°	+0.6200	+0.6345	270°	+0.6300	+0.6345
120°	−0.2438	−0.2049	300°	−0.1696	−0.2049
150°	−1.3282	−1.3448	330°	−1.3451	−1.3448
172°22′	−1.6319	−1.6870†	352°22′	−1.7221	−1.6870†

* Maximum. † Minimum.

values given in Table 3 are then obtained and compared with author's values. It will be noted that by the use of the author's extremely unwieldy equations, numerical values are obtained which differ only little from those obtained from Equations (106), and the writer is inclined to believe that the latter are just as "exact" as the formulas given by Mr. Silverman; they certainly are "exact enough" and very much simpler.

[96] See, also, *Transactions*, Am. Soc. C. E., Vol. 101 (1936), Equations (17) and (21), pp. 1247-1248.

AMERICAN SOCIETY OF CIVIL ENGINEERS

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DISCUSSIONS

FLOW CHARACTERISTICS IN ELBOW DRAFT-TUBES[1]

Discussion

BY JEROME FEE, ASSOC. M. AM. SOC. C. E.

JEROME FEE,[19] Assoc. M. Am. Soc. C. E. (by letter).[19a]—This important paper is distinguished by the experimental technique displayed and the excellent form in which the results are presented. Apart from the special field of draft-tube design, the study is one of great interest in connection with general problems of hydraulics.

The author refers to spiral flow as an important factor. Because of its significance in these experiments and its bearing on diverse subjects, the writer is prompted to discuss at some length the author's "Theory of Induced Spirals in Pipe Bends." Despite the fact that many attempts have been made to explain the phenomena of spiral flow, the writer feels that, even now, a complete and satisfactory analysis is lacking. The purpose of this discussion is not to propose a new explanation, but to call attention to imperfections in the theories which have been advanced from the time of James Thomson (21),[8] in 1876, to Albert Einstein,[20] in 1933, or L. Prandtl,[21] in 1935. It is astonishing how frequently this subject has been referred to in scientific literature and how many outstanding men of science have given it their attention.

There is a close similarity between Thomson's solution and that of Einstein. The author's theory seems to be based on the same general ideas, but the relationship is not so apparent.

Referring to flow in river channels, Thomson writes (21)[8]:

"But the layer of water along the bottom, being by friction much retarded, has much less centrifugal force in any bar of its particles extending across the river; and consequently it will flow sidewise along the bottom towards the inner bank, and will, part of it at least, rise up * * *."

NOTE.—The paper by C. A. Mockmore, M. Am. Soc. C. E., was published in February, 1937, *Proceedings*. Discussion on the paper has appeared in *Proceedings*, as follows: April, 1937, by F. T. Mavis, M. Am. Soc. C. E.

[19] Hydr. Engr. Designer, San Francisco Water Dept., San Francisco, Calif.

[19a] Received by the Secretary March 15, 1937.

[8] Numerals in parenthesis refer to corresponding numbers in Appendix I of the paper, *Proceedings*, Am. Soc. C. E., February, 1937, p. 283.

[20] "The World as I See It", p. 111.

[21] "Aerodynamic Theory", Vol. III, p. 161 (Guggenheim Fund Series, W. F. Durand. Editor in Chief).

Einstein describes the circulating motion of water stirred in a cup, and likewise finds that the velocity near the bottom and, hence, the centrifugal force, is reduced, from which he concludes that a circulatory motion will occur in radial planes, that is, normal to the general whirling of the water. The writer wishes to take no undue liberties and would prefer to give Professor Einstein's exact words, but is restrained by the formidable phrasing of the copyright clause. It should be mentioned that Einstein was writing for non-technical readers.

It can scarcely be conceived that a reduction in centrifugal force, by itself, will cause a particle to move radially inward toward the center of a curved path. That would be equivalent to saying, "because of a reduction in the force of gravity the particle started to rise." Nevertheless, this method of explaining spiral flow is quite common. To cite one additional example, Richard Hein[22] writes:

> "The layers of water adjacent to the upper and lower sides [actually the top cover and bottom of a closed vortex chamber] experience a breaking effect due to the skin friction, and the resulting reduction in centrifugal force causes flow towards the center of the vortex."

Directing attention next to the explanation given by the author in the paragraphs following Equation (5) where he states that "the pressure at Point *d* will be less than at Point *b*," this opinion appears to be based on the inference that, because of the proximity of the channel walls, the velocity is reduced; consequently, the centrifugal force and, therefore, the pressure are reduced. The writer would expect a high, not a low, pressure at Point *d* in conformity with the usual rule that high pressures go with low velocities. The validity of this rule is proved in an interesting manner by comparing Figs. 6 and 7.

Of the various explanations of spiral flow which the writer has examined, that of L. Prandtl[23] seems the nearest approach to a correct solution. The writer regrets that certain of Prandtl's references are not available, particularly those of which he writes: "The results of experiments recently performed, or about to be performed, should, after some time, permit the definite quantitative determination of the magnitudes involved in the phenomena just described."

Prandtl finds that two distinct cases are involved. One, as the writer understands it, may be illustrated by considering the flow in a curved open channel with frictionless sides, but a rough bottom. In this case, the pressure gradient at the bottom is determined by the pressure gradient of the more freely flowing filaments immediately above. The differential pressure on a bottom particle, not being opposed by as high a centrifugal force as for those above, causes the particle to more radially inward.

In the other case, the curved open channel should be thought of as having a frictionless bottom, but rough curved sides. Prandtl considers

[22] "Investigation of the Thoma Counter Flow Brake", by Richard Hein, *Transactions*, Munich Technical Univ., *Bulletin 8*, pub. in 1935 by Am. Soc. of Mech. Engrs.

[23] "Aerodynamic Theory", Vol. III, p. 161.

that the particles close to the convex wall, by having their velocity reduced, are subject to lower centrifugal forces than adjacent filaments and tend to remain close to the wall. On the other hand, particles at the concave wall, likewise subject to smaller centrifugal forces than adjacent particles, will be pressed away from the wall by such faster moving particles.

Prandtle expresses these ideas with such brevity that the writer feels justified in giving his interpretation, for which he alone is responsible. The writer may have misconstrued or misunderstood the matter, but he is unable to see how spiral flow follows as a result, although the argument for the first case seems the more convincing of the two.

The situation in regard to spiral flow seems to parallel a similar problem in aerodynamics where it is necessary in order to explain "lift" to refer to a "circulation" of air about an airplane. To account theoretically for this circulation has been a matter of great difficulty. In a perfect fluid, circulation about an airplane wing would be impossible. In a real, viscous fluid, to account for circulation by the simple statement that the velocity is reduced in the boundary region is quite inadequate.

The writer is of the opinion that all attempts to explain the circulation of water flowing in bent pipes (which circulation, of course, is the distinguishing component of spiral flow), without reference to vortex filaments and surfaces of discontinuity of flow, will ultimately be rejected. The use here of the word, "vortex," has reference not to the entire phenomena of circulation, but to the localized vortices which sometimes determine the nature of the entire flow.

One very interesting feature of the author's study appears to have been the formation of two distinct types of spiral flow. In one type the motion is in circular paths with the fastest moving particles, neglecting longitudinal flow, farthest from the center, or approximately so. This is the type of circulation, as the writer understands it, which Einstein and Thomson describe, and which may exist as one single circulation or two circulations combined, as shown in Fig. 1. On the other hand, such spiral flow as that illustrated in Bend No. 5, Fig. 9(*b*), of sufficient intensity to cause vibration, must be the better known "free vortex" flow in which the velocities increase, approaching the center.

To revert to the first-mentioned type, the writer has before him the typewritten notes recently used in a college class in hydrodynamics. A description of the flow in pipe bends states that "the circulation corresponds closely to that of the Rankine combined vortex, the tangential velocity component being greatest at a point between the center line and the pipe wall, and decreasing toward the center and wall." This is the only specific reference the writer has seen to the distribution of velocities of circulation in pipe bends. It is clear that the distribution differs from that of the free vortex.

These lecture notes explain the circulation by stating that "since these centrifugal forces diminish as the velocity approaches zero, the pressure in

the boundary region at the outside of the bend is less than that just within this boundary layer." The writer believes that this is incorrect, as it implies that pressure may manifest itself only as a consequence of centrifugal force.

In connection with the author's deriviation of the equation for potential flow, the equation of continuity expressing zero divergence should not be regarded as a specification for irrotational flow.

No one interested in the subject of hydraulics can fail to appreciate the remarkable value, from both the theoretical and practical standpoint, of Professor Mockmore's study.